

A
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AND

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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers in the present Volume, are Sir H. C. Englefield, Bart. M. P. F. R. S.; Mr. H. Steinhauer; Mr. John Gough; Ra. Thicknesse, Esq.; Mr. Wm. Wilson; C. Wilkinson, Esq.; J. Whitley Boswell, Esq.; Mr. W. Brandé; Aletes; Mr. John Dalton; Capt. J. Mortlock; W. F. S.; Mr. Charles Sylvester; Mr. Ezekiel Walker; T. B. C.; Jos. Huddart, Esq. F. R. S.; E. O.; W. B.; Mr. N. Mendelshon; C. L.; Mr. Accum; Right Hon. Sir Joseph Banks, Bart. P. R. S. &c. &c.; Mr. R. Matthews; Mr. J. Biddle; An Old Correspondent.—Of Foreign Works, Thenard; Hesinger; Bergelius; Morozzo; Bucholz; Dartigues; Schorbering; Boullay; Lagrange; Lusac; Erman.—And of English Memoirs, abridged or extracted; Smithson Tennant, Esq. F. R. S.; W. H. Wollaston, M. D. F. R. S.; C. Hatchett, Esq. F. R. S.; Gregory Watt, Esq.; Right Hon. C. Greville, V. P. R. S.; George Pearson, M. D. F. R. S.; J. Hinckley, Esq.; F. S. A.; T. A. Knight, Esq.

Of the Engravings the Subjects are, 1. An ancient Egyptian Engraving in Symbols, supposed to be astronomical. 2. Figures to illustrate the Galvanic Charge, by C. Wilkinson, Esq. 3. Outlines of Pendulous Measures of Time, by J. W. Boswell, Esq. 4. Figure to illustrate Mr. Gough's Theory of the Augmentation of Sound, by communicated Vibrations. 5. Instrument for surveying, by Capt. J. Mortlock. 6. Instrument for taking Designs from Nature. 7. Plan and Sections of an improved Air-Pump, by Mr. Mendelshon. 8. Figures to illustrate Mr. Gough's Theory of the

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the Speaking Trumpet. 9. Improvement in Woulfe's Apparatus. 10. Method of drawing a Line in a Circle equal to the Side of its Square. 11. Two quarto Plates, exhibiting highly magnified Representations of the Parasitic Plan which causes the Blight in Corn, from Drawings by M. Bauer, Botanical Painter to the King. 12. Mathematica Diagrams of the Properties of Triangles, by Mr. Gough. 13. Application of Spectacle Glasses, by Mr. E. Walker.

Soho Square, April, 1805

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A
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AND
THE ARTS.

JANUARY, 1805.

ARTICLE I.

Concerning the original Inventors of certain philosophical Discoveries: the Reflexion of Cold; Compression of Water in a metallic Vessel; the Telescope; and a perspective Instrument formerly described. In a Letter from Sir H. C. ENGLEFIELD, Bart. M. P. F. R. S. &c. &c.

To Mr. NICHOLSON.

SIR,

ALTHOUGH abstractedly speaking, it is of no importance to science who first made the most valuable experiments, or to what individuals the world is indebted for the most useful discoveries; yet as the sense of mankind has universally in this, as in several other things, been in direct opposition to frigid reasoning, and every one has been desirous to vindicate the fame of eminent men, by ascertaining to them the honour due to their labour or genius, I trust that the following observations will not be deemed unworthy a place in your valuable Journal.

1. The experiment of the reflexion of cold has been, as far as I know, both in conversation and in printed books, universally ascribed to Mr. Pictet of Geneva. Now in the collection

Universal sentiment in favour of discoverers.

Reflexion of cold ascribed to Pictet.

It was before
made by the
Acad. del Ci-
mento.

tion of experiments published by the Academy Del Cimento, of Florence, in the year 1666, the identical experiment is described in the following words :

“ Ninth Experiment.

Narrative.

“ We were desirous to try whether a concave speculum, exposed to a mass of ice weighing 500 pounds, would reflect any sensible degree of cold on a very delicate thermometer of 400 degrees, placed in it focus. The result was, that the thermometer instantly sunk : but a doubt remained whether the thermometer was acted on more by the direct cold of the ice, or that reflected by the speculum. This doubt was removed by covering the speculum : and certain it is (whatsoever might be the cause) that the spirit instantly began to rise again. Yet still we will not presume positively to affirm, that this rise might not have been owing to some other cause than the taking off the reflection from the speculum, all the precautions not having been taken which might be considered necessary to secure absolute assent to the experiment.”—*Saggi di Naturali Esperienze*, page 176.

Excellency of
their publica-
tion.

I do not mean to assert that Mr. Piſcet had seen this most excellent book, or that he borrowed from it without acknowledging his obligation ; but the honour of having first made the experiment is certainly due to the Florentine philosophers. It may not be foreign from the subject here to observe, that the “ *Saggi*,” for perspicuity, brevity, and that diffident caution so essential to the investigation of truth, is a model of philosophical writing, not perhaps excelled by any book written since that time, and more admirable when we consider the diffuse and obscure style so much in fashion in the works of the learned at that period.

The Acad. del Cimento said, in most books, to have compressed water in a globe of gold.

2. Scarcely any treatise on natural philosophy has failed to quote an experiment made by the academicians Del Cimento, on the incompressibility of water inclosed in a globe of gold, and submitted to the pressure of a screw press. Mr. Canton, in his experiments on the same subject, published in the 62d vol. of the *Phil. Trans.* so speaks of it. You will probably be surprised when I assert, that the academicians did not try the experiment with a gold globe, but one of silver ; and that they give a reason for using silver in preference to gold, namely, that the ductility of gold was so great that it would extend

The globe was silver, and compressed by hammering.

extend its dimensions by pressure, and so defeat the intention of the experiment. Neither was the globe compressed by a machine, but merely by hammering.

But the honour of this experiment is not due to the Florentine philosophers, but to our own illustrious countryman Lord Bacon, who, in the *Novum Organum*, Book II. Sect. 45, gives the very same experiment on water enclosed in a large and strong globe of lead, which being first compressed by the hammer and then by a screw-press, the water exuded through the pores of the metal, and stood in a dew on its surface.

Lord Bacon first made this experiment.

The inaccuracy with which this experiment has been related, though in this case of no great importance, may however be an useful lesson, and induce those who wish to attain to truth, to trust as little as possible to information at second-hand, but to recur to the original authority whenever it is practicable to do so.

Before I conclude, permit me to mention a curious circumstance relative to the æra of the invention of the telescope. Almost all books place it in the year 1609, and the first discoveries of Galileo were made in 1610; yet Kepler, in his account of the comet which appeared in September, 1607, (*See Kepler de Cometis, page 25*) expressly states, that though, to the naked eye, it was only equal to certain fixed stars near it, yet, when viewed with telescopes (*perspicillis*), it appeared larger than those stars.

The telescope was used by Kepler in astronomy two years before Galileo.

Although the intercourse between the different nations of Europe was at that time much less than it now is, it is very difficult to suppose that Galileo should not, in upwards of two years, have heard, not only of the invention, but its application to celestial observations by a man so eminent in science as Kepler: and it is scarcely less extraordinary that Kepler himself should not, in that space of time, have forestalled Galileo in some of those discoveries which rendered his name so illustrious; for however imperfect Kepler's telescope might be, it is hardly possible that it could have failed to shew him the satellites of Jupiter, which are visible to the commonest opera glass.

Remarks on the progress of discovery, &c.

I am, Sir,

Your obedient servant,

H. C. ENGLEFIELD.

Filney Street, Nov. 30, 1804.

Ramden was the inventor of a telescopic perspective instrument referred to.

P. S. I beg leave to add, that the machine for drawing in perspective, described in your Journal for October, page 122, was (I am almost absolutely certain) the invention of the late most eminent Mr. Ramden, and was made by him full 25 years since, for the Right Honourable Charles Greville.

II.

Account of an ancient Egyptian Sculpture with Hieroglyphics, supposed to relate to Astronomy. In a Letter from Mr. H. STEINHAEUSER. With an Engraving.

To Mr. NICHOLSON.

SIR,

Fulneck, Oct. 25, 1804.

Porphyry sculptured in the form of a scarabæus, and bearing hieroglyphics.

PERHAPS the liberty that I take in submitting the annexed draught of an Egyptian antiquity, may be an intrusion upon the plan of your valuable Journal. The Egyptian hieroglyphic inscriptions are involved in such impenetrable darkness, that every trifle contributed to their elucidation cannot but be acceptable, indeed equally acceptable to the antiquarian, the historian, and the astronomer; since the greater part have probably a reference to that science. Stones sculptured in the shape of a scarabæus are, I believe, (for I make no pretensions to the title of an antiquarian) well known; there are a few in the British Museum; but I never yet saw either a facsimile of the inscription, or any attempt at an explanation of the same. The drawings inclosed are taken from a stone brought by the Swedish consul at Aleppo to Stockholm, and there presented by him to a gentleman, who has favoured me with the loan and permission to make drawings or impressions of it at pleasure. The upper part, of the size of the sketch, obviously represents a scarabæus; probably the S. Sacer, Lin. & Fabr. though the indentures on the head are but slightly marked, and a slight damage between the thorax and elytra render it impossible to discover whether a scutellum be indicated or not. It was perhaps the only beetle they represented in sculpture, and the accuracy of a modern nomenclator is consequently unnecessary to determine it. If I recollect right,

this

this beetle was made use of as a symbol of the annual rotation of the sun. The under side is flat, or slightly convex, perhaps from wear, and bears eight lines of hieroglyphics, probably referring to the change of the seasons. The drawing, made with the greatest accuracy from an impression in sealing-wax, is consequently inverted.* Should you, sir, or any of your friends, wish for one to prove the exactness of the draught, it is very much at your service. The stone itself is apparently a species of porphyry, the *serpentino verde antico*, of a green mass, with very dark green field-spar, and pale green indistinct stripes. The plainness of the inscription, and the same symbol occurring several times upon it, led me to suspect, that it would be no very difficult task, for such as are versant in antiquities of the kind, to discover somewhat of its purport; and being conscious of its value, I was unwilling to let it lie by me, without offering it to the attention of such as are more deeply interested in it. Your wish to promote the arts and sciences, which so obviously pervades your esteemed Repository, makes me hope that you will excuse mine, in case the subject should not be admissible into your Publication.

I remain, Sir,

Your obedient humble servant,

H. STEINHAEUER.

P. S. Is the carbonate of barytes a frequent British mineral, except at Chorly? If not, it may perhaps be interesting information, that it is found in very large quantities near Murton in Cumberland, and some other places of the vicinity. The native oxide of zinc, scarcely inferior to the flowers of zinc of the shops, I do not recollect to have seen mentioned. It is found in one part of the West Riding of Yorkshire, in considerable quantities.

* The engraver having followed the drawing upon the copper, our print is not inverted.—N.

III.

Extract of a Memoir on the fuming Liquor of CADET.

*By Cit. THENARD.**

History of the
discovery of
Cadet's fuming
liquor, &c.

CADET discovered this liquor near half a century ago, in making investigations on arsenic. The name of its author was given to it then, which it has retained ever since, because its intimate nature and constituent principles were unknown. The thick smoke which this singular product spreads through the air, its specific gravity, greater than that of water, its oily state, its great volatility, its powerful odour, its spontaneous inflammation in the air, observed by Cadet and the chemists of Dijon; in fine, all its properties, each more extraordinary than the other, induced Cit. Thenard to subject it to analysis.

Method of ob-
taining it.
Distillation of
acetite of potash
and arsenious
acid.

He began by procuring several ounces of this liquor, by distilling, according to Cadet's process, equal parts of acetite of potash and arsenious acid, the produce of which he received in glass globes, cooled with a mixture of ice and marine salt. A liquor, very little coloured, and smelling strongly of garlic, soon passed into the receivers; at the same time much gas was disengaged, which spread the same odour, and the receivers were filled with vapours, so heavy that they seemed to run like oil. When the operation was terminated, he unluted the apparatus, and broke the retort. The bottom of it was covered with a white, acrid, and alkaline substance of potash, arising from the acetite employed; and the neck was lined with crystals of arsenic, owing to the reduction of the arsenious acid. The gases, the quantity of which was very great, contained arseniated hydrogen, in addition to the carbonated hydrogen and carbonic acid, given by all vegetable matters which are decomposed by fire. The liquid product was composed of two very distinct strata, holding metallic arsenic in solution, which was not long before it was deposited in flakes; the upper one was of a brownish-yellow and aqueous, the lower one was less coloured, and of an oily appearance. He separated them, by pouring them into a tube drawn to a point by the lamp, which allowed him to receive them

Residue.

The liquid product consists of
two distinct fluids.

* From Bulletin des Sciences, Tom. III. p. 202.

in different vessels. The heaviest, as being the most useful to be known, was first examined. At the commencement he was affected by the thick vapours which it disperses through the air, and by its extremely penetrating and horribly fetid odour. Its action on the animal economy is so powerful, that he found it impossible to devote more than one hour in a day to his enquiries, and he was more than once tempted to abandon them. He was in the same state as if he had taken a strong medicine, and experienced stupefying effects; against which he employed sulphurated hydrogen dissolved in water, with success.

Examination of the heaviest.

It powerfully affected the animal economy.

As he had but a small quantity of the liquor at his disposal, and it was of importance that his attempts should not be fruitless, he regulated the order of his enquiries in this manner: He first determined the cause of the odour which it spreads through the air; then he sought for that of the thick vapours which it produces, and afterwards that of its spontaneous inflammation; and he made use of the determination of these three points to discover the fourth, and most important, the constituent principles of the substance.

Order of the investigation.

The odour could only proceed from the substance itself, or else from an elastic fluid which it might hold in solution, and which the author supposed to be arseniated hydrogen. He therefore distilled, with great care, a certain quantity of the liquor in a small glass retort, to which were adapted a receiver and a tube for collecting the gases. He obtained nothing but the air of the vessels; the liquor was entirely volatilized, and passed into the receiver without having undergone any alteration, except that its colour was not quite so deep. Hence the odour of the arsenical liquor is owing to its property of being volatilized, and, probably, dissolved in the air.

Cause of the odour.

The liquor was unchanged by distillation.

The cause of its vapour could only be owing to an absorption of oxygen, or to an absorption of the water dissolved in the air, or to both these effects at the same time. The air of a flask, into which Cit. Thenard had poured a few drops, immediately lost its transparency, and, in a short time, became incapable of supporting the combustion of a taper. A vessel of the same size, filled with carbonic acid, offered the same phenomena, but in a less distinct manner. To guard against the contact of the air, he had been careful to suspend a very thin tube, containing the liquor, to the cork of the flask,

Its vapour is occasioned by an absorption of oxygen and water.

so

so that he could easily break it against the sides of the vessel.

The vapours were not perceptible when he used carbonic acid perfectly dried; whence he concluded, that the vapours of the arsenical liquor are owing to an absorption of the oxygen and the water contained in the air jointly, but that the first of these causes seems to be more powerful than the second.

Its property of inflaming spontaneously is owing to metallic arsenic.

From this it should seem that the arsenical liquor is possessed of the property of inflaming alone. It does not however take fire at the approach of a body in combustion, when it is very pure; and it is observed, that, in all the spontaneous inflammations which it experiences, the combustion always begins at the black specks which disturb its transparency, and are only metallic arsenic very much divided.

It must therefore contain arsenic,

It now remained to determine the nature of the arsenical liquor. Its odour, which resembles arseniated hydrogen gas, indicated that it must contain arsenic, and that this metal must have great influence on the phenomena which it offers: Its combustibility, its consistence, and its appearance, indicated an oily matter; and, although it did not change the tincture of turnsole, and no re-agent demonstrated immediately the existence of acetous acid, this body must nevertheless be expected in it. To succeed in insulating these different substances, the author tried the alkalies; but experience soon convinced him that he must have recourse to other means.

an oily matter,

and acetous acid; but this was not indicated by alkalies.

Oxygenated muriatic acid decomposed it.

He employed the oxygenated muriatic acid with much greater success. Some drops of the liquor, poured into this gas, were instantly inflamed, and their decomposition was complete. They were then precipitated in white flocks by lime-water, and in yellow ones by sulphurated hydrogen; while, on being saturated with potash and evaporated, they formed a foliated salt, strongly attracting the humidity of the air, acrid, sharp, decomposable by sulphuric acid, and disengaging a strong odour of vinegar. The quantity of arsenic and of acetous acid obtained, being far from corresponding with the quantity of liquor which had been used, there must therefore have been another body in it which it was requisite to insulate, and this was accomplished by treating a new portion of the liquor with a quantity of water sufficient to dissolve it; then on decomposing it by sulphurated hydrogen, it yielded a precipitate, slightly yellow, very much divided, formed principally of arsenic and sulphur, which was a considerable time in separating

Formed a foliated salt with potash.

Its solution in water, decomposed by sulphurated hydrogen, yielded sulphur, arsenic, and oil, containing acetous acid.

rating

CADET'S FUMING LIQUOR.

rating from an oil, which was afterwards seen swimming on the surface of the liquor. This contained a great deal of acetous acid. Its decomposition may be further facilitated by exposing it to the air; then it emits thick vapours, it is crystallized, and becomes slightly humid. It is rendered turbid by lime-water, and yields a yellow precipitate with sulphurated hydrogen.

From these different experiments it follows, that this liquor is composed of oil, acetous acid, and arsenic, nearly in a metallic state; and that it must be considered as a species of soap, with a base of acid and arsenic, or as a sort of oleo-arsenical acetite.

Composition of the heaviest liquor.

This analysis was very useful in that of the upper liquor. In fact, notwithstanding the difference which seems to exist between them, since the latter resembles water, can combine with it in all proportions, forms only a slight cloud in the atmosphere, has much less odour, and does not inflame in any case, it is easy to prove that it only differs from the first by its greater proportion of acetous acid, and by the water which it contains; for it reddens the tincture of turnsole strongly, effervesces with the carbonates, gives rise to acetites, and is slightly precipitated of a yellow colour by sulphurated hydrogen, which separates a little oil from it. A very small quantity of oxygenated muriatic acid speedily destroys the odour, and it is then precipitated white by lime-water, and of a deep yellow by the hydro-sulphurets. Exposure to the air produces in it, by time, the same changes as take place immediately with oxygenated muriatic acid. Finally, a liquor, exactly similar, is formed by dissolving a few drops of the lower liquor in very weak vinegar, and thus synthesis confirms the results of analysis.

The upper fluid differs from the lower, only by having a larger proportion of acetous acid and water.

We can now establish a theory, clear of all hypothesis, on the phenomena offered by the distillation of acetite of potash and arsenious acid; we see that one part of the arsenious acid is entirely reduced; that another is only brought near the metallic state; that the acetite of potash is totally decomposed; that almost all the acetous acid is also decomposed; and, that from these different decompositions result water, carbonated hydrogen, arseniated hydrogen, carbonic acid, a peculiar oil, oxide of arsenic, arsenic, and potash; that the potash forms the white residue found in the vessels in which the distillation

Theory of the process.

Component parts of the fuming liquor.

was

was performed; that the arsenic is sublimed, and adheres to the neck of the retort; that the three different species of gas are mixed, and may be collected in flasks; finally, that the water, the oil, the acetous acid, and the oxide of arsenic, are condensed in the receiver; that these three last bodies, by combining in certain proportions, form a very volatile compound, heavier than water, and sparingly soluble in it; and that it is for this reason that it separates into two very distinct strata; the lower of which must be considered as an oleo-arsenical acetite, and the other as a portion of the first dissolved in water, which solution is promoted by an excess of acetous acid.

IV.

Account of Cerium, a new Metal found in a mineral Substance from Bastnas, in Sweden. By W. D'HESINGER and J. B. BERGELIUS.

(Concluded from Page 300, Vol. IX.)

With Succinic Acid.

Succinate of cerium.

(P.) AT first, succinic acid renders the saturated solutions of cerium turbid, but afterwards they become clear. Thus, a few drops of succinate of ammonia, poured into a saturated nitric or muriatic solution of this metal, occasion a precipitate, which soon disappears. On pouring in a greater quantity of this re-agent, the succinate of cerium is immediately deposited. This combination is white; it is not wholly insoluble in water, since the solution from which it was separated still retains a little, as is manifested by evaporation, or by alkalies. The salt obtained by digesting free succinic acid with the oxide, comports itself in the same manner. The acid dissolves it readily. Exposed to the fire, it burns with a blue flame.

Since the acetate of cerium is not precipitated by the succinate of ammonia, this is a certain means of obtaining the cerium freed from iron.

With Gallic Acid.

Gallate of cerium.

(2.) If crystallized gallic acid is put into a saturated solution of muriate of cerium, a small quantity of a white precipitate is

is formed. The alkalis augment it, and give it a clear chocolate colour. If a larger quantity of alkali is gradually added, the quantity of the precipitate, and the intensity of its colour are increased; in proportion as this addition is made, the precipitate becomes of a reddish brown, and at length, by exposure to the light, gives a turbid, deep green solution.

With Prussic Acid.

(R.) The saturated solutions of cerium are precipitated white by the prussiates. The voluminous precipitate has much resemblance to silver precipitated by muriate of soda. An excess of acid readily dissolves this precipitate. Prussiate of cerium.

Oxide of Cerium with Sulphurated Hydrogen.

(S.) At the commencement, the saturated solutions of cerium are precipitated of a brownish colour by hydro-sulphuret of ammonia; but if more of this re-agent is added, the precipitate is of a deep green. The muriate of cerium alone takes a deep green colour, but if an alkali is added to it, an hydro-sulphurated combination of muriate of cerium, at a *minimum* of oxidation, is formed, which is rapidly precipitated of a bright green colour. Hydro-sulphuret of cerium.

The hydro-sulphurated cerium, well dried, has a deep green colour, almost black. It is easily destroyed by heat; put into a crucible, moderately hot, it burns with a yellowish, phosphoric flame, only visible in the dark.

Sulphurated Oxide of Cerium.

(T.) The sulphuret of ammonia gives a fading brown precipitate with the solutions of cerium. This sulphuret, added in excess, gives a precipitate of a grass green, which becomes bright green by desiccation: it burns with a blue flame and the pure oxide remains. Sulphurated oxide of cerium.

The muriate of cerium at a *minimum* is precipitated white by sulphuret of ammonia.

Oxide of Cerium with Phosphorus.

(U.) A piece of pure phosphorus was put into a solution of muriate of cerium contained in a closed vessel, and kept for several days on a stove. The bottom and sides of the vessel were covered with a white precipitate, and the piece of phosphorus Phosphate of cerium.

phorus was covered with a hard brown crust, from which the phosphorus was separated by heating it in warm water. This crust was tenacious, and shining in the dark. Heated, it took fire like phosphorus, and left a residue of a small quantity of oxide, which, on being melted with borax, exhibited the characters described above, except that the globule retained its clear green colour, after being cooled. It appears probable that these phenomena arose from the phosphorus containing cerium. But the phosphorus, kept for a month in the same solution, slowly deposited a white powder; whence it is uncertain whether the formation of the crust, mentioned above, was owing to the cerium. The acid was always in excess in the solution, and the white powder had all the appearance of phosphate of cerium.

With the Alkalies.

Action of the
alkalies.

(V.) The pure alkalies do not dissolve cerium, even by fusion. By this means it is easily deprived of manganese. Pure ammonia digested with the oxide, does not dissolve it, but renders it yellowish.

The carbonated alkalies dissolve the oxide of cerium in small quantity. The solution is yellow, and is precipitated by the acids. They also dissolve it by fusion in covered vessels. In the open fire, it oxides too much to enter into solution.

Iron or zinc do
not precipitate
the solutions of
cerium.
Action of gal-
vanism.

The solutions of cerium are not precipitated by iron or zinc.

The electric pile of Volta only decomposes the salts, and the yellow oxide adheres to the conductor.

V.

*New Experiments on Absorption by Charcoal, made by Means of
a new Machine. By C. L. MOROZZO.*

(Concluded from P. 264 Vol. IX.)

On Oxygen Gas.

Oxygen gas.

25th. I NEXT proceeded to the examination of the absorption by charcoal in oxygen gas.

I therefore extracted some oxygen gas from red precipitate to be employed in my different experiments. Of this gas,
trite

tried with charcoal in my machine, eight inches and six lines Oxygen gas. were absorbed: this great absorption surprized me; for in my first experiments, in 1783, made in closed tubes, I had had but a very small one: I therefore found no difficulty in attributing it to my matras having been melted towards the close of the operation, which made me suspect that my gas had been spoiled by the nitrous acid; and I repeated the experiment with great care.

26th. I then took oxygen gas which had been obtained from red precipitate with great attention, and had been prepared in the laboratory of Doctor Bonvoisin; what was my astonishment at observing that, in thirty-six hours, the absorption amounted to twelve inches and eight lines, and that it continued for several days: in forty-eight hours it was fourteen inches six lines; in four days, fifteen inches two lines; in five days, sixteen inches four lines; and, finally, in three days more, the gas was entirely absorbed.

27th. This experiment was too interesting to omit repeating it with the greatest possible precision.

I therefore took some of the same oxygen gas, and the absorption was twelve inches three lines in the first four hours; in forty-eight hours, it was thirteen inches six lines; in three days, fourteen inches five lines; at the end of the fourth day, it was fifteen inches three lines; of the fifth, sixteen inches; of the sixth, sixteen inches seven lines; of the seventh, seventeen inches two lines; finally, on the eighth day the gas was entirely absorbed.

28th. I repeated the same experiment on oxygen gas obtained from water exposed to the sun, in which I had put three ounces of charcoal-powder*; in this gas, which was very pure, the absorptions took place in the same manner.

29th. These facts will surprise those who, having made their experiments on oxygen gas in tubes closed hermetically, and by passing the charcoal through the mercury, obtained only very small absorptions, which never exceeded three inches.

* In the twelfth volume of the Italian Society, I have inserted a memoir on the property which charcoal mixed with water has of developing one-third more of oxygen gas, of a superior quality to that obtained from pure water: In this memoir I prove, that a part of this oxygen gas is produced by the charcoal, which communicates the *principle of fire* to the air contained in the water.

three

Oxygen gas. three lines in tubes of twelve inches in height. I am certainly not mistaken; and M M. Rouppe and Van-Norder, who have repeated my experiments in their apparatuses, have obtained the same results, and have found, as I did, that, next to hydrogen, oxygen gas is that which is least absorbed by charcoal.*

30th. I must not pass over a singular experiment, the explanation of which will also be very difficult.

I examined the absorption which a piece of charcoal, that had remained seven hours in the bright light of the sun, had effected in the same oxygen gas; it was only between seven and eight lines, while a similar piece, which had been only five hours in the solar light, produced an absorption of ten inches three lines in carbonic acid gas.

It must be observed, that a piece of charcoal exposed to the light of the sun and placed in oxygen gas, produced only an absorption of seven or eight lines, just like the absorption of a piece which had been exposed to the sun's light, and placed in hydrogen gas, which gave only an absorption of six lines: and, in this, these results are perfectly conformable to my experiments, made in the year 1783.

31st. This difference of the results is therefore only owing to the method either of passing the charcoal under the mercury, or of leaving it in the machine. It appears that, by passing it through the mercury, the charcoal loses much of its attractive power, which it retains in the machine.

Let us endeavour to find whether these differences can be accounted for.

32d. It might be supposed that the red-hot charcoal inflames the oxygen gas; but since I do not open the key until some time after the charcoal has been inserted, it does not appear to me to be likely that the gas can be inflamed: besides, if this were the case, the absorption would be made at once, and it would not require eight days to be complete.

33d. It might be supposed that, in this case, the charcoal supplied hydrogen gas, which, mixing with the oxygen gas and producing water, would cause the absorption: but during the whole experiment there was not the smallest drop of water perceived in the tube.

* Ann. de Chimie.

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GASES. ABSORBED. BY CHARCOAL.

34th. There might be reason to believe that the charcoal, by inflaming the oxygen gas, might convert it into carbonic acid gas, which is the most absorbed of all the gases; but to satisfy myself of this, I produced an absorption of oxygen gas by a piece of charcoal, as in the former experiments: in twenty-three hours it amounted to twelve inches, then with a syringe of crystal I passed some lime-water through the mercury into the remaining gas, and it did not become turbid, which proves that carbonic acid was not formed; and, having turned up the tube, the residual gas extinguished a taper: I believe it to have been azote.

35th. I was of opinion that incandescent charcoal would absorb more than another piece which had been suffered to cool till it no longer appeared red. Experiment shews a greater absorption in the first for twenty-four hours; but, leaving the apparatus undisturbed, in two days the absorption became equal. I operated upon atmospheric air.

36th. I must acknowledge my inability to give an explanation of these singular experiments, as I have already declared above: were I to hazard any conjectures, they would probably be overturned by new theories, which do not spare even the labours of the greatest chemists in France.

37th. I shall therefore continue my operations as I proposed, and new experiments will, at least, supply the want of systems.

On the Difference in the Charcoals made use of.

38th. Having ascertained the absorptions effected by the charcoal of beech wood in atmospheric air and in the different gases, I proceeded to examine the variations which the charcoals of different woods would produce in them. Difference in the charcoals made use of.

39th. I therefore took two pieces of charcoal, the one of beech, the other of the branches of fallow deprived of bark: I introduced them both in an incandescent state, and examined the difference of their absorption in the same atmospheric air. Charcoal of fallow;

That of beech gave an absorption of seven inches and eight lines in six hours.

That of fallow gave only an absorption of four inches and three lines, and there was no further absorption after six hours.

These

These experiments correspond perfectly with those which I have published in my third memoir. I shall dispense with the relation of several other experiments which I have made on different woods, such as hazle, vine-twigs, &c. all of which gave less absorptions.

of box ;

I afterwards tried the charcoal of box wood, although it is very compact wood ; but since Kirwan found that this wood contains much more saline matter than any other wood, it was desirable to submit it to experiment ; its absorption, however, was considerable, and equal to that produced by beech-wood. The greater or less quantity of saline matter in the wood of which the charcoal is made, therefore, does not contribute to the variations in the absorption.

of cork.

40th. I afterwards examined the charcoal of cork, which interested me greatly, because Cit. Odier, from the experiments of Dr. Beddoes, has proposed it as a medicine : I was desirous of examining whether it contained more of the principle of fire than the others. I therefore charred cork ; but it is so light, that, after the charring, a piece of equal bulk with that of the beech which I had used, weighed only three grains : to bring the circumstances to an equality, it was therefore necessary to make the experiment with a piece of beech charcoal which weighed three grains* : it was in atmospheric air it was tried. The following are the results :

Three grains of beech charcoal produced an absorption of one inch and nine lines.

The charcoal from cork absorbed only nine lines.

The charcoal of cork contains more of the principle of fire than any other charcoal.

41st. According to my principles†, the charcoal of cork must contain the matter of fire more than any other charcoal ; thus when it is necessary to give it as a medicine, either inwardly or outwardly, it should be preferred to all the others ; and I am of opinion that it is by oxidating, and not by disoxidating, that it produces its astonishing effects. Besides, we have instances of substances which act in this manner. The oxygenated muriatic acid, far from abstracting the oxygen, is

* The pieces of beech charcoal which I use, and which weigh thirty-six grains, are of the length of ten lines, and about five in diameter.

† See my third memoir on charcoal.

supposed, on the contrary, to add a considerable quantity of it to fetid ulcers, and carries off their fetidity as well as charcoal.*

Water may sometimes be used in the Instrument instead of Mercury.

42d. Although, in the course of these experiments, I have constantly employed mercury, in some experiments a coloured water may be used with advantage, since it is better seen in the tube; but this cannot be used when the gases which are absorbed by water are examined, nor with the carbonic acid and the other acid gases which are much absorbed: then it is necessary to change the tube, and to substitute one which, instead of eighteen inches, is at least thirty-six in height; without which it will be entirely filled.

Coloured water may sometimes be substituted to mercury in the machine.

43d. It was on atmospheric air taken in my room with the windows open, that I experimented with coloured water. The charcoal produced an absorption of ten inches and a half. It has been seen, that, with the mercurial apparatus, I had had an absorption of eight inches some lines.

The pressure of the atmosphere in the water therefore augmented the absorption, which usually takes place with the mercury, by two inches and a half; that is to say, one-fourth of the height.

The pressure of the atmosphere affects the results.

44th. I should observe, that very frequently the small differences of one or two lines observed in the absorptions of the same quality of air or of gas, are owing to the variations of the atmosphere, which I ascertained by comparing the height of the barometer during the experiment.

The Pieces of Charcoal used in these Experiments, acquire Weight.

45th. After the experiments, all the pieces of charcoal employed had acquired greater weight: this augmentation was from half a grain to two grains, and appears to be dependent on the greater absorption.

The charcoal acquires weight in these experiments;

46th. I took four pieces of charcoal which had been used in the experiments made with atmospheric air, which, among them, had acquired four grains in weight; I passed them

* See the notes of M. Odier to Beddoes's memoir. Bibl. Brit. Vol. VI. p. 359.

under water, in a tube of one inch in diameter and six inches high, which was also filled with water. A part of the air which they contained was immediately developed, but I only obtained about two inches, (they had, however, absorbed among them nearly thirty-two inches). The reformed air was found to be composed of carbonic acid gas and azote gas. In this operation a part of the air is fixed in the charcoal to be embodied with it, and cannot be again dislodged but by employing fire. I believe these combinations frequently occur in nature, and the great Newton suspected it.

47th. I dried the pieces of charcoal which had been wetted in the preceding experiment, in the air: I afterwards examined them, by again heating them to redness in the fire, and placing them in the machine; they absorbed the same quantity of air as before.

No water produced by the diminution of the gas under examination.

48th. I directed all my attention, as well after as during the course of the experiments on atmospheric air, to see if I could perceive any drops of water either in the tube or in the apparatus, but I did not observe the least trace of it; neither was the charcoal moist; on the contrary, its surface was covered with ashes.

Experiments to be made by saturating the Charcoal with different Substances.

Experiments still to be made.

49th. Since, during the progress of an investigation, there are always new experiments which offer themselves, I perceived that it would be interesting to make trial with pieces of charcoal saturated with different saline and acid substances, as M. M. Rouppe and Van-Norden, of Rotterdam, had undertaken to do, and to see the effects they would produce, with my machine, as well in atmospheric air as in the different gases: But my memoir being of sufficient length already, I leave the care of these enquiries to other philosophers; and this the more willingly, since it appears that one of the most able chemists, M. Van Mons, has engaged in this subject, which, in his hands, will doubtless receive every degree of perfection which can be desired.

50th. I shall only give the result of four experiments which I made with atmospheric air.

I saturated pieces of charcoal with nitric acid, sulphuric acid, solution of potash, and lime-water; they were not used until

until they had been thoroughly dried on filtering-paper. I put them cold into my machine. The following are the results:

1st. In the first six hours the charcoal, saturated with nitric acid, produced an absorption of four lines and a half; afterwards it gave back the air, and even depressed the mercury below its level. Effects of charcoal saturated with nitric acid;

2d. The charcoal, saturated with sulphuric acid, effected an absorption of nine lines, which it preserved uniformly for twenty-four hours. with sulphuric acid;

3d. The charcoal, saturated with solution of potash, made an absorption of three lines, and remained so for twenty-four hours. with solution of potash; and

4th. That, saturated with lime-water, made only a small absorption of a line and a half, although it was kept in experiment for twenty-four hours. with lime-water.

These four experiments prove that charcoal, saturated with different substances, loses, in part, its power of absorbing: probably these solutions, by filling the pores of the charcoal, in some measure deprive it of this property. Charcoal loses its absorbent power by being saturated with some substance.

51st. No person can be more aware than I am, that my enquiry is far from being carried to that degree of perfection which I wished: I hope, nevertheless, that philosophers will not be dissatisfied that I have presented them with new experiments, and have made them acquainted with an instrument, by means of which they can appreciate the effects of solar light and caloric on charcoal; an instrument which, in many cases, will serve them for an eudiometer, and which, when azote shall be better known, will acquire greater precision. I hope that, by the labours of other philosophers who may employ themselves on this subject, this interesting branch of natural philosophy will be brought to perfection, and an explanation of these surprising phenomena be obtained.

VI.

Further Observations on the Constitution of mixed Gases. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

An apology for this letter.

OTHER philosophers, besides Mr. Dalton, have maintained the existence of free elastic vapour in the atmosphere; but as the modification given by this gentleman to the opinion, is perhaps the last it will receive, I am desirous of trespassing once more on your pages, in reply to my opponent, as well as to shew the changes which the present controversy has produced in his system.

Some objections stated by Mr. D. considered.

Mr. Dalton's last letter begins by exposing my ignorance of chemistry, and disapproving the data, by help of which air has been shewn to be a chemical compound. Although his suspicions may be just in many instances, they are wrong in this; for the facts stated by Mr. D. were known to me at the time of writing that paper; and the reprobated data were adopted for the following reasons:

Specific gravity of oxygenous gas.

Mr. Davy found the weight of 100 cubic inches of oxygenous gas, of the temperature of 50° , to be 35.06 grains, the barometer standing at 30 inches: now the densities of the same elastic fluid at 50° and 60° , are in the ratio of 105824 to 103744*: but when the magnitude is given, *i. e.* 100 parts, the weights are as the densities; consequently the weight of 100 cubic inches of oxygenous gas at 60° , is 34.37 grains, the barometer standing at 30: but 31 grains being the weight of an equal bulk of air in like circumstances, and its specific gravity being denoted by 1000, the corresponding specific gravity of oxygenous gas is expressed by the number 1108; which exceeds Mr. Kirwan's expression only by five. This approximation to an exact coincidence, affords the strongest evidence of the correctness both of Mr. Kirwan and Mr. Davy, that can be expected in a question of the kind; and, as the former gentleman made use of the same apparatus to determine the weight of air which had lost its oxygen by being exposed to

* Thomson's Chemistry, Vol. I. p. 342.

the martial paste of sulphur, the specific gravity, drawn from his experiment, was certainly to be preferred in my calculation to any other. This residuum was called by me azote, probably because Mr. Kirwan called it phlogisticated air; but it was evidently considered as a compound in the essay alluded to by Mr. Dalton; and certainly the advocate for a multiplicity of co-existent atmospheres will not controvert the supposition. The remaining objections made by my opponent to the paper in question, are levelled at my ignorance, not at my conclusions; they shall therefore be passed over in silence. In the mean time, Mr. D. perhaps will not take it amiss, if I request him to repeat his calculations with the specific gravity of oxygenous gas when properly corrected, and to try what will be the variation of the eudiometer, either on the data of Mr. Davy or M. Lavoisier.

Permit me, Mr. Nicholson, to conclude this part of my subject by observing, that an instrument of great use in pneumatics might be constructed on the principles of your hydrostatic weighing machine. The body of it should be of glass, furnished with proper stopples and stop-cocks, so that it might be easily exhausted and replenished. The preceding is but an imperfect sketch of an instrument, the mechanism of which I resign to your pen and pencil, should the idea appear worth pursuing.

Mr. Dalton's strictures on my last letter ascribe to me very singular opinions; but he neglects to point out the paragraph where the grounds of this charge may be found; a charge which insinuates, that, according to my notions of things, a vessel containing a cubic foot of one gas cannot receive an equal bulk of a different kind: Here indeed he accuses me of gross ignorance; for there are but few who do not know that fermenting liquors increase the density of the gases, which occupy part of the close vessels containing them. In reality, the charge is so perfectly groundless, that, had it been made by almost any other person, I would not have hesitated to have pronounced my accuser guilty of misrepresentation to conceal his want of argument, and to secure the prejudices of the superficial reader in his own favour. He may court the admiration of such; I do not desire their applauses.

Mr. Dalton speaks with confidence of what he calls his important argument, *i. e.* his assertion, that equal quantities of vapour all,

A new instrument recommended,

A misrepresentation pointed out.

Mr. D.'s important argument no argument at all.

vapour enter a vacuum and an equal space previously occupied by a gas. No one would refuse its due importance to this argument, after seeing or perusing a satisfactory demonstration; but I know of no such thing in Mr. Dalton's essays. He asserts, indeed, that a quantity of dry air, confined in a tube, receives an augmentation of force from the presence of water, equal to that which is produced by the same cause in the exhausted tube. But this is no demonstration, unless the porosity of air be first admitted; for, without this concession, Mr. D. has no right to estimate the quantity of vapour present by the spring of it; this ought to be determined by weight only. But when did Mr. D. weigh an exhausted vessel, and afterwards take its weight when filled with vapour? When has he repeated the same experiment upon the same vessel containing dry and moist air, and found the result favourable to his hypothesis? Negative answers to these questions must convince an impartial judge, that the porosity of the air has been again artfully introduced to establish its own existence: thus we find the fundamental datum of the system to be demonstrated by arguing in a circle, which begins and ends with the thing to be proved.

On the other hand, Mr. D. is convinced that the admission of vapour into an open bottle expels part of the air. This is a proof that steam, supposing its existence in the atmosphere, finds a space crowded with gases less convenient than a vacuum; it, therefore, makes room for itself, by diminishing the densities of these gases, and creates in them Mr. Dalton's capacity for the reception of water. This fact being established, it will also be evident, that, when vapour is forced into a gas confined in a close vessel, the former cannot make its way through the latter, otherwise than by condensing it: Such is the nature of the pores of the air, according to Mr. D.'s own principles. The atmosphere is also found to oppose a similar resistance to newly developed gases; for the gaseous compound contained in an open vessel filled in part with a fermenting liquor, is not a column of the atmosphere permeated by an additional body of the carbonic acid, but a mixture, in which the proportion of common air is comparatively small. This fact overturns Mr. D.'s general conclusion, that atmospheres may be added to the compound atmosphere at pleasure,

ture, or taken from it, without its disturbing the densities and situations of the remaining atmospheres*.

The free passage of gases through each other was once the fundamental maxim of my opponent; at which time he maintained, that the variations in the weight of the atmospheric compound arise from the changeable nature of the aqueous atmosphere, not from the permanent gases†. This dogma is essential to the system; it has, however, been abandoned by its author himself; for Mr. D. when attempting to explain the observation of Mr. Kirwan, is obliged to admit the permanent gases to be more abundant in dry than in moist air; that is, the densities and positions of these gases depend upon the state of the contained vapour, contrary to the hypothesis. This departure from the primitive maxim is avowed openly in Mr. D.'s explanation of the experiment with the moist bottle. Here he is compelled to confess, that steam, at low temperatures, distends the pores of air; and endeavours to preserve the existence of his aqueous atmosphere by something, which looks like a demonstration from the final Q. E. D.; but it ought to be remembered, that the particles of a fluid press equally in all directions; consequently, that the corpuscles of air would act with their full force on the contiguous corpuscles of his supposed vapour.

Perhaps Mr. D. will perceive, by this time, that logical precision has placed him in the circumstances of certain philosophers mentioned in his third essay, who are unable to defend their opinions. In fact, Mr. D.'s hypothesis is repugnant to natural appearances in its primitive form; for, according to it, a shower of rain is a prodigy; seeing the drops must displace equal bulks of air, and this removal must be brought about by the inadequate weight of a column of vapour which has lost part of its spring.

JOHN GOUGH.

Middlefham, December 13, 1804.

P. S. Since the publication of my paper on vegetation, I have found, by Dr. Thomson's Chemistry, that M. Ingenhouz had formerly made the same discovery. In 1795, I read

* Manchester Memoirs, Vol. V. p. 546.

† Manchester Memoirs, Vol. V. p. 547.

his work, *Sur les Végétaux*, printed at Paris, 1787; but undoubtedly this section had by some means been overlooked. All that I have to do at present, by way of reparation, is to thank Dr. Thomson for his information, and to declare that paper of no value; which in all probability will prove a sufficient apology to M. Ingenhouz.

VII.

On two Metals, found in the black Powder remaining after the Solution of Platina. By SMITHSON TENNANT, Esq. F. R. S. From the Philosophical Transactions for 1804.

Introduction,
with reference
to the experi-
ments made by
Descotils,

UPON making some experiments, last summer, on the black powder which remains after the solution of platina, I observed that it did not, as was generally believed, consist chiefly of plumbago, but contained some unknown metallic ingredients. Intending to repeat my experiments with more attention during the winter, I mentioned the result of them to Sir Joseph Banks, together with my intention of communicating to the Royal Society, my examination of this substance, as soon as it should appear in any degree satisfactory. Two memoirs were afterwards published in France, on the same subject; one of them by M. Descotils, and the others by Mellis, Vauquelin and Fourcroy. M. Descotils chiefly directs his attention to the effects produced by this substance on the solutions of platina. He remarks, that a small portion of it is always taken up by nitro-muriatic acid, during its action on platina; and, principally from the observations he is thence enabled to make, he infers, that it contains a new metal, which, among other properties, has that of giving a deep red colour to the precipitates of platina.

and by Vauque-
lin.

M. Vauquelin attempted a more direct analysis of the substance, and obtained from it the same metal as that discovered by M. Descotils. But neither of these chemists have observed, that it contains also another metal, different from any hitherto known.

Substance ob-
tained from the
grains of platina,

The substance with which my experiments were made, was obtained from platina which had been previously freed from the sand and other impurities generally mixed with it; so that

it

it must have been contained in the substance of the grains of platina. Though it has somewhat the appearance of plumbago, it may easily be distinguished by its superior weight. By weighing it in a phial with water, I found its specific gravity almost 10.7.

Before I describe the method of separating the two metals of which it consists, it may be worth while to mention the effects of it, when combined with different metals in its entire state. It readily unites with lead; but, even with ten times its own weight, the compound has not, when melted, much fluidity. Upon dissolving the lead in nitrous acid, the black powder was obtained, with little apparent alteration, not having been entirely broken down, but consisting chiefly of the same scaly particles as at first. With bismuth, zinc, and tin, the effects were nearly similar; but, by fusion with copper in a very strong heat, a more perfect union was produced. On attempting to dissolve the compound by nitro-muriatic acid, one of the powder was taken up with the copper, forming with copper; a very dark solution.

The undissolved portion consisted partly of the substance in its original form of scales, and partly of a blacker powder, the particles of which were too small to be visible, and which had probably been completely combined with the copper. This substance may be easily united, by fusion, with silver or gold; and it is particularly deserving of attention, that it cannot be separated from these metals, by the usual process of refining. It remains combined with either of them, after cupellation with lead; and with the gold, after quartation with silver. The alloys retain considerable ductility; and the colour of that with gold, is not materially different from pure gold.

I shall now proceed to describe the analysis of the black powder, and the properties of the two metals which enter into its composition. The method which I used for dissolving it, was similar to that employed by M. Vauquelin, the alternate action of caustic alkali, and of an acid. I put a quantity of the powder into a crucible of silver, with a large proportion of pure dry soda, and kept it in a red heat for some time. alkali being then dissolved in water, had acquired a deep orange, or brownish-yellow colour, but much of the powder remained undissolved. This powder, digested in marine acid, gave a dark blue solution, which afterwards became of a

resembling
plumbago, but
heavier.

It combines with
metals:—A
small portion
renders lead,
bismuth, zinc,
and tin, diffi-
culty fusible

silver or gold;
not separable by
refining.

Analysis of the
black powder;
which consists of
two metals.

Fusion with
soda.

Solution in wa-
ter, deep orange,
leaving a pow-
der;

which was partly
dissolved in mu-
riatic acid.

dusky

Fusion, &c. repeated on the undissolved residue.
Silex.
 dusky olive-green, and finally, by continuing the heat, of a deep red colour. Part of the powder being yet undissolved by the marine acid, was heated as before with alkali; and, by the alternate action of the alkali and acid, the whole appeared capable of solution. At each operation, some silex was taken up by the alkali; and, as this continued till the metallic part was entirely dissolved, it seems to have been chemically combined with it.

The alk. solution contains an oxide which separates by repose.
 The alkaline solution contains the oxide of a volatile metal, not yet noticed, but which I shall presently describe, and also a small proportion of the other metal. If this solution is kept for some weeks, the latter metal separates spontaneously from it, in the form of very thin flakes, of a dark colour.

The acid solution contains the same metal, but principally that examined by Vauquelin.
 The acid solution also contains both the metals, but principally that which has been mentioned by the French chemists. The properties of this last metal, which they have remarked, are those of giving a red colour to the triple salt of platina with sal-ammoniac, of not being altered by muriate of tin, and of giving, with pure alkali, a dark brown precipitate. M. Vauquelin also adds, that it is precipitated by galls, and by prussiate of potash; but I should rather ascribe these precipitates to some impurity, and probably to iron.

The name Iridium given to the metal examined by Vauquelin.
 As it is necessary to give some name to bodies which have not been known before, and most convenient to indicate by it some characteristic property, I should incline to call this metal *Iridium*, from the striking variety of colours which it gives, while dissolving in marine acid.

In order to obtain the compound of this metal with marine acid in a pure state, I tried to make it crystallize.

Crystals of its muriate
 By slow evaporation of the solution, only an imperfectly crystallized mass was produced; but this, being dried on blotting-paper, and dissolved in water, afforded, by again evaporating as before, distinct octaedral crystals. These crystals dissolved in water, gave a deep red coloured solution, inclining to orange. With an infusion of galls no precipitate was formed, but the colour was instantly, and almost entirely, taken away. Muriate of tin, carbonate of soda, and prussiate of potash, produced nearly the same effect. Pure ammonia precipitates the oxide; but (possibly from adding it in excess) I found it retained a part in solution, acquiring a purple colour. The pure fixed alkalis also precipitate the greater part of the oxide.

dissolved in water.

Precip. by alkalis and metals.

oxide, but are capable of retaining a part in solution, becoming of a yellow colour. All the metals which I tried, excepting gold and platina, produced a dark or black precipitate from the muriated solution, which is at the same time deprived of its colour. The iridium may be obtained in a pure state, merely by exposing the octaedra crystals to heat, which expels the oxygen and the muriatic acid. It appeared of a white colour, and was not capable of being melted, by any degree of heat I could apply. I could not combine it with sulphur, nor with arsenic. Lead easily unites with it; but is separated by cupellation, leaving the iridium upon the cupel, as a coarse black powder. Copper forms with it a very malleable alloy, which, after cupellation with the addition of lead, left a small proportion of the iridium, but much less than in the former case. Silver may be united with it, and the compound remains perfectly malleable. The iridium was not separated from it, by cupellation, but occasioned on the surface a dark or tarnished hue. It appeared not to be perfectly combined with the silver, but merely diffused through the substance of it, in the state of a fine powder. Gold alloyed with iridium is not freed from it by cupellation, nor by quartation with silver. The compound was malleable; and did not differ much in colour from pure gold, though the proportion of alloy was very considerable. If the gold or silver is dissolved, the iridium is left, in the form of a black powder.

The metal iridium is white, infusible, and rejects sulphur and arsenic.

Its habitudes with the metals.

The yellow alkaline solution, which I have already mentioned as containing a metallic oxide, distinct from the former, is considered by M. Vauquelin as a solution of the oxide of chrome in alkali; but I could not, by any test, discover the presence of chrome. After the superfluous alkali had been neutralized by an acid, it produced a pale or buff-coloured precipitate with a solution of lead, and not the bright yellow which is given by chrome. But, as we are indebted to the above distinguished chemist, among many other important discoveries, for our knowledge of the existence of chrome, it is not improbable that some kinds of platina may contain that substance, besides the other bodies usually mixed with it. When the alkaline solution is first formed, by adding water to the dry alkaline mass in the crucible, a pungent and peculiar smell is immediately perceived. This smell, as I afterwards

The alkaline solution did not contain Chrome.

It contains the volatile oxide of a metal called Osmium;

wards

wards discovered, arises from the extrication of a very volatile metallic oxide; and, as this smell is one of its most distinguishing characters, I should on that account incline to call the metal *Osmium*.

expellable from
the alkali by an
acid;

This oxide may be expelled from the alkali by any acid, and obtained in solution with water by distillation. The sulphuric acid, being the least volatile, is the most proper for this purpose; but as, even of this acid, a little is liable to pass over, a second slow distillation is required, to obtain the oxide perfectly free from it. The solution thus procured is without colour, has a sweetish taste, and the strong smell before mentioned. Paper stained blue with violets, was not changed by it to red; but, by being exposed to the vapour of it in a phial, the paper lost much of its blue colour, and inclined to gray. As a certain quantity of this oxide is extricated during the solution of the iridium in marine acid, that part may also be obtained by distillation.

or obtained by
distilling the
black powder
with nitre.

Another mode by which the oxide of osmium may be obtained in small quantity, but in a more concentrated state, is, by distilling with nitre the original black powder procured from platina.

Process de-
scribed.

With a degree of heat hardly red, there sublimes into the neck of the retort, a fluid apparently oily, but which, on cooling, concretes into a solid, colourless, semitransparent mass. This, being dissolved in water, forms a solution similar to that before described. The oxide, in this concentrated state, stains the skin of a dark colour, which cannot be effaced.

Most striking
character of so-
lution of osmi-
um:

The most striking test of the oxide of osmium, is an infusion of galls, which presently produces a purple colour, becoming soon after of a deep vivid blue. By this means, the presence of this, and of the metal first described, may be observed,

Habitudes with
various bodies;

when the two are mixed together. The solution of iridium is not apparently altered by being mixed with the oxide of osmium; but, on adding an infusion of galls, the red colour of the first is instantly taken away, and soon after the purple and blue colour of the latter appears. The solution of the oxide of osmium with pure ammonia, becomes somewhat yellow, and slightly so with carbonate of soda. It is not affected by pure magnesia, nor by chalk; but with lime a solution is formed, of a bright yellow colour. The solution with lime gives with galls a deep red precipitate, which becomes

becomes blue by acids. It produces no effect on a solution of platina or gold; but precipitates lead of a yellowish-brown, mercury of a white, and muriate of tin of a brown colour.

The oxide of osmium becomes of a dark colour with — alcohol; alcohol, and after some time, separates in the form of black films, leaving the alcohol without colour. The same effect is produced by ether, and much more quickly.

This oxide appears to part with its oxygen to all the metals, — the metals. excepting gold and platina. Silver being kept in a solution of it for some time acquires a black colour; but does not entirely deprive it of smell. Copper, tin, zinc, and phosphorus, quickly produce a black or gray powder, and deprive the solution of all smell, and of the power of turning galls of a blue colour. This black powder, which consists of the osmium in a metallic state and the oxide of the metal employed to precipitate it, may be dissolved in nitro-muriatic acid, and then becomes blue with infusion of galls.

If the pure oxide of osmium, dissolved in water, is Amalgam with shaken with mercury, it very soon loses its smell; and the mercury. metal, combining with the mercury, forms a perfect amalgam.

Much of the mercury may be separated by squeezing it through leather, which retains the amalgam of a firmer consistence. The remaining mercury being distilled off, a powder is left, of a dark gray or blue colour, which is the osmium in its pure state. By exposing it to heat with access of air, it evaporates, with the usual smell; but, if the oxidation is carefully prevented, it does not seem in any degree volatile. Being subjected to a strong white heat, in a cavity made in a piece of charcoal, it was not melted, nor did it undergo any apparent alteration. Heated in a similar situation with copper and with gold, it melted with each of these metals, forming alloys which were quite malleable. These compounds were easily dissolved in nitro-muriatic acid, and, by distillation, afforded the oxide of osmium with the usual properties.

The pure metal which has been previously heated, does not seem to be acted on by acids; at least I could not perceive any effect produced by boiling it for some time with nitro-muriatic acid. By heating it in a silver cup with caustic alkali, Pure osmium not soluble in acids; but easily with alkali.

alkali, it immediately combined with the alkali, and, with water, gave a yellow solution, similar to that from which it was procured. Acids expel from this solution the oxide of osmium, which has the usual smell, and the power of giving to infusion of galls the blue colour before mentioned.

VIII.

Remarks upon certain Observations of Mr. WILKINSON, respecting Galvanism. By RA. THICKNESSE, Esq.

To Mr. NICHOLSON.

SIR,

Wigan, Dec. 17, 1804.

Observations re-
specting gal-
vanism, &c.

THE remarks of Mr. Wilkinson (which I have not had an earlier opportunity of attending to) on my letter of the 20th September, on galvanism, are in general founded only on misconception; the consequence, probably, of a hasty perusal in a public room. He attributes to me an assertion, "that two metals are requisite to the production of galvanic phenomena;" but my words are, and particularly marked by italics, "two metals, *or other substances*;" and were intended to signify two metals, or two other substances; or one metal and one other substance; and to include even that which may be dissolved in the water. The experiment, therefore, of La Grave, with brain and muscle, which Mr. W. adduces, is not an exception; and "that a single metal suffices" perfectly with pure water only, he did not, I fancy, wish to be understood.

That copper and the other negative metals are, as I expressed, "acted upon by the hydrogen," I must refer to Mr. W. for authority, to pages 177 and 178 of your Journal; and that particularly silver (my words are copper, or silver) is rendered more brittle, to page 85: but there cannot, surely, be stronger evidence that copper has an affinity for hydrogen, and consequently an influence in the decomposition of the water, than is afforded by the experiments in which hydrogen gas is obtained from the copper of the pile, and oxygen gas from the oxidated zinc; part of the hydrogen, and of the oxygen, being prevented from uniting with, and borne away from the metals by the electricity.

Mr.

Mr. W. imputes to me also the mistake of having "ob-
 served that the sensation is in proportion to the surface acted
 on;" adding, that "the experiments of the French philoso-
 phers proved, that the action of galvanism on animal sub-
 stances is in the ratio of the number of plates employed, and
 not the surfaces exposed." This is another instance of the
 inaccuracy of Mr. W.'s observations; for sensation I never
 mentioned; nor action on animal substances; not at all con-
 sidering the intensity, but the quantity only of the electric fluid
 produced; which may not always correspond; for the charge
 of small plates being weak, although according to their sur-
 faces, the electricity meets with almost insurmountable ob-
 structions from the interstices of the pile; which to a stronger
 charge, from larger plates, are very trifling impediments:
 but Mr. W. thinks, himself, that "the production of galva-
 nic phenomena is always proportionate to the degree of oxi-
 dation;" and must not the quantity of metal oxidated be in
 proportion to the surface exposed to the water? The French
 philosophers, too, have "concluded (I quote Mr. W.'s own
 words, from p. 207 of your last volume) that the effects of a
 galvanic battery on metallic substances, are in proportion to
 the surfaces of the plates employed:" Must we not then sus-
 pect the experiments to have been imperfect which led them
 to conclude, if they have so concluded, that the effects on
 animal substances are different?

Whether I "set out too hypothetically as to electricity
 being a modification of caloric," is a question on which many
 of the most celebrated men of science would differ from Mr.
 W. in opinion, "*Pars invenit utraque Causus*; yet it appears
 to me, that the experiment noticed by a *Correspondent*, p. 173,
 almost determines it: "If the gas which is produced from one
 of the wires communicating with the pile in the water, be
 united and inflamed with the other in a just proportion, the
 water which is common to both is reproduced, and *common*
fire in great abundance." Thus the electric fluid, which alone
 conveys the oxygen and hydrogen from the water, actually be-
 comes, when differently modified by combustion, for there
 is "no evidence of the presence of fire until this point of
 time," active caloric.

But supposing, with Mr. W. that the electric matter and
 caloric are perfectly distinct bodies, we have still the same
 reason

Observations re-
specting gal-
vanism, &c.

reason to conclude that the former is combined with water, which we have to believe that it is contained in metals: What then becomes of it when the water is decomposed, if, as he imagines, the electric, or "galvanic phenomena, entirely depend upon the metal undergoing a chemical change," I must confess I cannot conjecture: but, in truth, it is entirely the cause of all the phenomena which take place: the quantity afforded by the metal, so very small a portion of which undergoes any chemical change, must be too trifling to be considered; for if any more than the oxidated part of the metal contributed, the quantity of the metal in the plates, as well as the extent of its surface, would in some degree have an effect: but this, as I alledged, is not the case; and, therefore, Mr. W.'s fanciful conjecture, that there is as it were a spring of electric fluid in the metal, as a resource for a supply, although very ingenious, may be regarded as a perfect hypothesis—a supposition unsupported by any fact; and as an instance, from his partiality for it on this occasion, of our disposition to overlook imperfections in our own performances, whilst we observe them in the works of others.

I am, Sir,

Your most obedient servant,

RA. THICKNESSE.

IX.

Abstract of a Memoir on the Possibility of obtaining Prussiate of Potash free from Iron; the Unalterability of the Prussic Acid at high Temperatures; and the true Nature of the Combinations of this Acid with different Bases. By BUCHOLZ. "

(Concluded from p. 282, Vol. IX.)

General deduc-
tions-

THESE two experiments therefore prove, that from four parts of dried blood and one of carbonate of potash, no more prussiated alkali can be obtained than from two of blood and one of alkali.

From what has been so far stated the author concludes, after varying and repeating the experiments here detailed,

1. That

1. That four parts of blood to one of carbonate of potash are ^{Best proportion} too much, and that equal parts of blood and carbonate of potash ^{of carbonate of potash and dried} are rather good proportions for obtaining the most possible ^{blood, are 1 p.} quantity of prussiated alkali. The latter proportions have, ^{and 2 b.} moreover, the disadvantage, that the excess of alkali acts upon the oxide of iron of the blood as well as upon the crucible, and therefore introduces new impurities. Two parts of blood and one of carbonate of potash, appear to be a better proportion for obtaining the greatest possible quantity of prussiate of potash, under like circumstances.

2. Alkalies deprived of carbonic acid, are not better than ^{It is no advantage to use a pure alkali.} carbonated alkalies for the production of prussiated alkalies by treatment with blood; for they become reconverted into carbonated alkalies during the process.

3. No pure prussiate of potash can be obtained, except care ^{The formation of pure prussiate requires a gradual heat without fusion, and no longer than till the flame ceases.} be taken that the mixture of blood and alkali be neither too much nor too little ignited. The criterion which may serve as a guide to the operator, is the cessation of the flame, after a gradually increased ignition without fusion. For if the mass ^{is heated suddenly so as to effect a partial fusion, or if it be continued after the disappearance of the flame, it will be in vain to look for pure prussiated alkali.} be heated suddenly so as to effect a partial fusion, or if it be continued after the disappearance of the flame, it will be in vain to look for pure prussiated alkali.

4. The high colour of prussiated alkalis is not always owing ^{When the high colour of p. alk. arises from charcoal, it does not produce fallacy.} to the presence of iron, but more frequently to a minute quantity of charcoal. This impurity is not capable of yielding results that could mislead the chemical enquirer in the application of this re-agent.

5. The quantity of water employed for the lixiviation of ^{Very little water should be used, and heat avoided.} prussiated alkali from the ignited mass, should be as small as possible, and heat should be avoided as much as possible. For if prussiated alkalis be heated in contact with water, part of the prussic acid is liberated, ammonia is produced, and carbonate of potash formed.

6. Acetic acid cannot be employed for removing the ^{Acetic acid cannot be used to take off the redundant potash.} mixture of carbonate of potash from prussiated alkalis; for the energy of the affinity of prussic acid is less than the energy of the affinity of carbonic acid to potash; the union of the prussic acid and potash is therefore demolished, in preference to that of the carbonate of potash.

X.

On a new Metal, found in Crude Platina. By WILLIAM HYDE WOLLASTON, M. D. F. R. S. From the Philosophical Transactions, 1804.

Introduction.

NOTWITHSTANDING I was aware that M. Descotils had ascribed the red colour of certain precipitates and salts of platina, to the presence of a new metal; and although Mr. Tennant had obligingly communicated to me his discovery of the same substance, as well as of a second new metal, in the shining powder that remains undissolved from the ore of platina; yet I was led to suppose that the more soluble parts of this mineral might be deserving of further examination, as the fluid which remains after the precipitation of platina by sal ammoniac, presents appearances which I could not ascribe to either of those bodies, or to any other known substance.

New metal,
Rhodium.

My inquiries having terminated more successfully than I had expected, I design in the present Memoir to prove the existence, and to examine the properties, of another metal, hitherto unknown, which may not improperly be distinguished by the name of *Rhodium*, from the rose-colour of a dilute solution of the salts containing it.

Palladium found
in ore of platina.

I shall also take the same opportunity of stating the result of various experiments, which have convinced me, that the metallic substance which was last year offered for sale by the name of Palladium, is contained (though in very small proportion) in the ore of platina.

Remaining solution after precipitation of platina, contains iron and other metals.

The colour of the solution that remains after the precipitation of platina, varies, not only according to its state of dilution, but also according to the strength and proportions of the nitric and muriatic acids employed. This colour, though principally owing to the quantity of iron contained in it, arises also in part from a small quantity of the ammoniaco-muriate of platina, that necessarily remains dissolved, and from other metals contained in still smaller proportions.

Precipitable by
zinc or iron.

(A. 1.) To recover the remaining platina, as well as to separate the other metals that are present from the iron, I have in some experiments employed zinc, in others iron, for their precipitation. The former appears preferable; but, when the latter has been used, the precipitate may immediately be freed from

from the iron that adheres to it, by muriatic acid, without the loss of any of those metals which are at present the subject of inquiry.

(A 2.) Having in one instance dissolved such a precipitate in The same residual solution
nitro-muriatic acid, and precipitated the platina by sal ammo- affords crystals
niac, I suffered the remaining fluid to evaporate without heat; by spontaneous
and obtained a mixture of various crystals, very different from evap.
each other in form and colour. From these, I selected for examination some that were of a deep red colour, partly in thin plates adhering to the sides of the vessel, and partly in the form of square prisms having a rectangular termination.

(A 3.) A portion of these crystals being heated in a small Deep red crystals
tube, yielded sal ammoniac by sublimation, and left a black left a white
residuum, which, by greater heat, acquired a brilliant metallic metal by heat,
whiteness, but could not be fused under the blowpipe. Having —not platina.
obtained this substance from a distinctly crystallized salt, I was inclined to consider it, as a simple metal; and, as I found it to be wholly insoluble in nitro-muriatic acid, I judged it not to be platina.

(A 4.) The crystals also, instead of being nearly insoluble, The crystals
like the ammoniaco-muriate of platina, were dissolved in a small were not of platina,
quantity of water, and gave a rose-coloured solution. Upon which makes the
mixing this with a solution of platina, the ammonia was trans- salts of platina
ferred by superior affinity to the latter, forming an ammoniaco- red.
muriate of platina; and the precipitate was of a yellow colour. Consequently, the metal contained in the salt, was neither platina nor that which gives the red colour to the salts of platina.

It would be useless to detail my first unsuccessful experiments The base was the
made upon the properties of this metal, in hopes of discovering metal rhodium
means by which its separation from platina might be effected; now first no-
I shall therefore confine myself to the following process, which ticed.
appears to be the most direct for procuring rhodium in a state of purity. In the same process also palladium is obtained, so as to afford a presumption, that it is rather a natural simple body, than any artificial compound.

(B 1.) Since the platina to be procured in this country, Process for ob-
generally contains small scales of gold intermixed, as well as a taining it.
portion of the mercury which the Spaniards employ for the Purification of
separation of the gold, the platina used for my experiments, the platina
after being by mechanical means freed, as far as possible, from, grains.

all visible impurities, was exposed to a red heat, for the purpose of expelling the mercury. It was then digested for some time in a small quantity of dilute nitro-muriatic acid, and frequently shaken, till the whole of the gold was dissolved, together with any impurities that might superficially adhere to the grains of platina.

Solution of the ore in nitro-mur. acid.

(B 2.) Of the ore thus prepared, nearly $2\frac{1}{2}$ ounces were then dissolved in nitro-muriatic acid, (diluted for the purpose of leaving as much as possible of the shining powder,) and the whole suffered to remain in a moderate sand heat, till completely saturated.

Precipitation by sal amm.

(B 3.) Such a portion of this solution was then taken for analysis, as corresponded to 1000 grains of the prepared ore. An ounce of sal ammoniac was next dissolved in hot water, and used for the precipitation of the platina. The precipitate obtained was of a yellow colour, and, upon being heated, yielded 815 grains of purified platina.

Residual liquor precipitated by zinc.

(B 4.) The water used for washing this precipitate having been added to the solution poured from it, a piece of clean zinc was immersed in it, and suffered to remain, till there appeared to be no further action upon the zinc. The iron contained in the ore (to the amount of 14 or 15 per cent.) remained in solution. The other metals had subsided, in the form of a black powder, which I estimated between 40 and 50 grains; but, as there was no occasion to weigh it with accuracy, I thought it better not to dry this precipitate, for, if it be heated, the rhodium is in danger of being rendered insoluble.

Precipitate deprived of copper and lead by nitric acid; and then dissolved (most part) in nitro-m. acid.

(B 5.) As I had previously ascertained that this precipitate would contain platina, rhodium, the substance called palladium, copper, and lead, the two last metals were first dissolved in very dilute nitric acid, aided by a gentle heat. The remainder, after being washed, was digested in dilute nitro-muriatic acid, which dissolved the greater part, but left as much as $4\frac{1}{2}$ grains undissolved.*

Addition of common salt and evap. left soda-muriate of platina, palladium and rhodium. A. but the triple salt of rhodium was washed off by alcohol.

(B 6.) To the solution were added 20 grains of common salt; and, when the whole had been evaporated to dryness with a very gentle heat, the residuum, which I had found, from prior

* It was presumed that this residuum consisted principally of the metal called by Mr. Tennant Iridium; but, as it was accidentally mislaid, and was not examined, it might also contain a portion of rhodium.

experi-

experiments, would consist of the soda-muriates of platina, of palladium, and of rhodium, was washed repeatedly with small quantities of alcohol, till it came off nearly colourless. There remained a triple salt of rhodium, which by these means is freed from all metallic impurities.

(C 1.) This salt, having been dissolved in a small quantity of hot water, and let to stand 12 hours, formed rhomboidal crystals, of which the acute angle was about 75° . Solution in water.

(C 2.) It was then again dissolved in water, and divided into two equal portions. Of these, one was decomposed by a piece of zinc, and the other examined by the following re-agents. One half precip. by zinc.

(C 3.) Sal ammoniac occasioned no precipitation; but, when a solution of platina was added to the mixture, a precipitate was immediately formed, and the colour of this precipitate was yellow; which again proves that the metal contained in this salt, is neither platina itself nor that which gives the red colour to its precipitates. The other half was precip. by platina, but not by sal-ammon.

(C 4.) Prussiate of potash occasioned no precipitation, as it would have done, if the solution had contained palladium. nor by pr. potash,

(C 5.) Hydro-sulphuret of ammonia, which would have precipitated either platina or palladium, caused no precipitation of this metal. nor by hydro-sulph. of ammonia.

(C 6.) The carbonates of potash, of soda, or of ammonia, occasioned no precipitation; but the pure alkalis precipitated a yellow oxide, soluble by excess of alkali, and also soluble in every acid that I have tried. nor by carb. alkalis; but by pure alk. soluble in acids.

(D 1.) The solution of this oxide in muriatic acid, upon being evaporated, did not crystallize; the residuum was soluble in alcohol, and of a rose colour. Sal ammoniac, nitre, or common salt, caused no precipitation from the muriatic solution; but formed triple salts, which were not soluble in alcohol. Muriatic solution of rhodium.

(D 2.) The solution in nitric acid also did not crystallize. A drop of this solution, being placed upon pure silver, occasioned no stain. On the surface of mercury a metallic film was precipitated, but did not appear to amalgamate. The metal was also precipitated by copper and other metals, as might be presumed, from the usual order of their affinities for acids. Nitric solution.

(E 1.) The precipitate obtained by zinc (C 2.) from the remaining half of the salt, appeared in the form of a black powder, weighing, when thoroughly dried, nearly 2 grains, corresponding to about 4 grains in the 1000 of ore dissolved. The precip. before made by zinc was black.

(E 2.)

simply heated,

(E 2.) When exposed to heat, this powder continued black; with borax, it acquired a white metallic lustre, but appeared infusible by any degree of heat.

fusible with
arsenic and with
sulphur.
Metallic button
not malleable,

(E 3.) With arsenic, however, it is, like platina, rendered fusible; and, like palladium, it may also be fused by means of sulphur. The arsenic, or the sulphur, may be expelled from it by a continuance of the heat; but the metallic button obtained does not become malleable, as either of the preceding metals would be rendered by similar treatment.

unites with
metals except
mercury.

(E 4.) It unites readily with all metals that have been tried, excepting mercury; and, with gold or silver it forms very malleable alloys, that are not oxidated by a high degree of heat, but become incrustated with a black oxide, when very slowly cooled.

Gold and rhodium
4 to 1.

(E 5.) When 4 parts of gold are united with one of rhodium, although the alloy may assume a rounded form under the blowpipe, yet it seems to be more in the state of an amalgam than in complete fusion.

Gold and rhodium
6 to one,

It does not dis-
colour gold.

(E 6.) When six parts of gold are alloyed with one of rhodium, the compound may be perfectly fused, but requires far more heat than fine gold. There is no circumstance in which rhodium differs more from platina, than in the colour of this alloy, which might be taken for fine gold, by any one who is not very much accustomed to discriminate the different qualities of gold. On the contrary, the colour of an alloy containing the same proportion of platina, differs but little from that of platina. This was originally observed by Dr. Lewis. "The colour was still so dull and pale, that the compound (five to one) could scarcely be judged by the eye to contain any gold."*

I find that palladium resembles platina, in this property of destroying the colour of a large quantity of gold. When one part of palladium is united to six of gold, the alloy is nearly white.

Solubility of
rhodium affected
by combination.

(E 7.) When I endeavoured to dissolve an alloy of silver or of gold with rhodium, the rhodium remained untouched by either nitric or nitro-muriatic acids; and, when rhodium had been fused with arsenic or with sulphur, or when merely heated by itself, it was reduced to the same state of insolubility. But,

* Lewis's Philosophical Commerce of Arts, p. 526.

when

when one part of rhodium had been fused with three parts of bismuth, or of copper, or of lead, each of these alloys could be dissolved completely, in a mixture of two parts, by measure, of muriatic acid, with one of nitric. With the two former metals, the proportion of the acids to each other seemed not to be of so much consequence as with lead; but the lead appeared on another account preferable, as it was most easily separated, when reduced to an insoluble muriate by evaporation. The muriate of rhodium had then the same colour and properties, as when formed from the yellow oxide precipitated from the original salt. (D 1.)

(E 8.) The specific gravity of rhodium, as far as could be ascertained by trial on so small quantities, seemed to exceed 11. That of an alloy consisting of one part rhodium and about two parts lead, was 11,3; which is so nearly that of lead itself, that each part of this compound may be considered as having about the same specific gravity. Specific gravity.

F. As it was expected that the alcohol employed for washing the salt of rhodium (B 6.) would contain the soda-muriates of platina and of palladium, the platina was first precipitated by fal ammoniac. This precipitate was of a deep red colour; and, when it had been heated, to expel the fal ammoniac, the platina which remained was of a dark gray colour. The alcohol before used in washing the salts examined.

(G 1.) To the remaining solution, after it had been diluted to prevent any further precipitation of platina, I added prussiate of potash, which instantly occasioned a very copious precipitate, of a deep orange-colour at first, but changing afterwards to a dirty bottle-green, which I ascribed to iron contained in the prussiate.

(G 2.) This precipitate, when dry, weighed $12\frac{1}{2}$ grains. After it had been heated, it left a metallic residuum, in small grains, of a gray colour, weighing nearly 7 grains. A small portion of it being heated with borax, communicated a dark brown colour to the borax, as from iron, and acquired a bright metallic lustre, but could not be fused under the blowpipe. With sulphur, however, it fused immediately into a round globule, which, by floating upon mercury, appeared of less specific gravity than that metal.

(G 3.) The whole quantity was then treated in the same manner, and purified by cupellation with borax, till it cooled with a bright surface. From the globule the sulphur was expelled,

pelled, by exposure to the extremity of the flame; and it became spongy and malleable, weighing in this state very nearly five grains.

Palladium.

(G 4.) A portion of this metal was dissolved in strong nitrous acid, was precipitated by green sulphate of iron, and in other respects, possessed all the properties ascribed to the palladium offered for sale, in the printed paper that accompanied it, as well as others since noticed by Mr. Chenevix.

Its differences from platina.

(G 5.) In its precipitation by prussiate, it differs most essentially from platina; and consequently is by no means difficult to be distinguished, or separated from it.

(G. 6.) The action of muriate of tin upon the solutions of these metals, is also totally different. A dilute solution of platina, is thereby changed from a pale yellow to a transparent blood-red. A solution of palladium, on the contrary, usually becomes opaque, by the formation of a brown or black precipitate; but, if mixed in such proportion as to remain transparent, it changes to a beautiful emerald-green.

(G 7.) In the formation of triple salts with the alkalis, as observed by Mr. Chenevix, palladium may be said to resemble platina; but the salts thus formed are far more soluble than the corresponding salts of platina, and differ entirely, in the colour and form of the crystals.

(G 8.) The soda-muriate of palladium is a deliquescent salt; that of platina, on the contrary, forms permanent crystals.

(G 9.) The triple salts of platina, with either muriate of ammonia or of potash, form octaedra crystals of a yellow colour, that are very sparingly soluble in water. The corresponding salts of palladium, likewise resemble each other in every respect. The crystals are very soluble in water, but insoluble in alcohol; their form is that of a four-sided prism, and they each present a curious contrast of colour, that certainly is not observable in any known salt of platina.

Curious contrast of colour in the salts of palladium.

(G 10.) Although the solution is of a deep red, the crystals are of a bright green when viewed transversely. In the direction of their axes, however, the colour is the same as that of the solution; but, on account of its extreme intensity, it is with difficulty distinguished in fragments that exceed $\frac{1}{100}$ of an inch in thickness. One consequence of this colour is nevertheless

very

very observable; namely, that in viewing any crystal obliquely, it appears of a dull brown, that arises from a mixture of the red and green.*

The characters of palladium that have been enumerated, undoubtedly belong to none of the simple substances that we are acquainted with; and no experiment that I have made, has tended to confirm the suspicion of its being a compound, consisting of any known ingredients. The experiments above related, show evidently, that the ore of platina contains a very small quantity of palladium; and it is not unlikely that this may have been a constituent part of some of the compounds obtained by Mr. Chenevix, and may have misled him, by some properties which he would consequently observe, into the supposition that he had formed palladium.

Probability that Mr. Chenevix was misled to infer that palladium is a compound, from its being found in platina.

It is not, however, without having repeatedly endeavoured to imitate his experiments, that I have ventured to dissent from such authority. I made many attempts to unite pure platina with mercury, by solution, and by amalgamation; but without success, in any one instance.

All attempts of the author to compare it were unsuccessful.

From a solution of platina, carefully neutralized, as Mr. Chenevix directs, with red oxide of mercury, and mixed with a solution of green sulphate of iron, I indeed obtained such a

* The change of colour above described, though certainly uncommon, is nevertheless not peculiar to the salts of palladium, but may be seen also in some kinds of tourmalin. Among those which come to us from Ceylon, some are transparent; and one variety is of a deep red in the direction of its axis, but of a yellowish green when viewed transversely. There is also a corresponding, but opposite contrast of colours, that has been observed by Muller, and described by Bergmann, in some of the Tyrolese tourmalins. The general aspect of these stones is black, and apparently opaque.—Some, however, of which the fracture is vitreous, are found to transmit a yellowish-red light when viewed transversely, but in the direction of their axis the colour is a dull bottle green.

In each of these tourmalins, as well as in the salts of palladium, the colour in the direction of the axis is at least ten times more intense than in the transverse direction. A thin lamina, cut from the end of a Tyrolese tourmalin for this purpose, transmitted no visible light, till it was reduced to $\frac{1}{86}$ of an inch in thickness; and, when less than $\frac{1}{100}$ of an inch, it was not more transparent than another portion of the same crystal seen transversely, $\frac{1}{10}$ of an inch in thickness.

precipitate

precipitate of metallic flakes as he describes; but, upon examination of these flakes, they yielded mercury by distillation; and the remainder consisted of platina combined with a portion of iron, but had not any properties which I could suppose owing to the presence of palladium.

Other facts and observations to show that palladium is a simple metal.

Upon comparing the specific gravity of this substance, which was said to be, at most, 11,8, with that of mercury or of platina, I was always strongly inclined to doubt the possibility of its being composed of these metals. I could recollect no one instance, in which the specific gravity of any compound is less than that of its lightest ingredient, and could not, without careful examination, admit the supposition, that mercury could be rendered lighter by intimate union with platina. It now appears fully confirmed that this persuasion, arising from uniform experience, was well founded; for, if we consider the difficulty of producing even an imperfect imitation of palladium, the failure of all attempts to resolve it into any known metals, the facility of separating it from any mixed solution of those which it has been supposed to contain, as well as the number and distinctness of its characteristic properties, I think we must class it with those bodies which we have most reason to consider as simple metals.

XI.

Letter from Mr. WM. WILSON, exhibiting the Electricity of Metals, without the Help of any condensing Instrument.

To Mr. NICHOLSON.

SIR,

London, Dec. 22, 1806.

Whether the electricity from metals alone be the effect of the contact or the separation,

WHEN I set about making the compound electrical condenser described in my last letter to you, I intended to repeat the experiments of Cavallo relating to the electricity obtained by the contact of metals related in the third volume of the fourth edition of his Treatise of Electricity; but before the instrument was finished, I was induced (by some experiments I had made relative to the cause of excitation of electricity) to suppose that it is not the contact of the metals that is the cause of the appearance of electrical signs, but the separation of the metals

metals from contact. And this supposition was very much strengthened, when, upon examination, I found that all who have made experiments on this subject have separated the metals from contact before they examined them as to their electricity.

☛ If the contact of the two metals be the cause of the electrical signs, the whole effect that one metal can have on another will be communicated at the time of contact, however few the points are that form the contact, because both the metals being conductors of electricity, if one possessed a greater proportion of it than the other, a part will be communicated to the other at the time of contact, to form an equilibrium, and this will be done as well by a few points of contact as by a great many. But if it is the separating them from contact, that is the cause of the electrical sign, the more extensive the contact is the more powerful will the signs be when the metals are separated.

To put this to the test of experiment, I pierced a piece of thin sheet copper full of small holes, just big enough to permit to pass through them two or three particles of filings of another metal at a time, so that almost every particle must be in contact with the copper before it can pass through, and consequently the surface of contact be very great with a comparatively small quantity of metal.

I sifted through this copper sieve some filings of zinc into a tin plate laid on the cap of a gold leaf electrometer, and the gold leaves diverged near an inch with positive electricity, when about half an ounce of filings had been sifted into it. Encouraged by this striking result, I procured sieves and filings of different metals. The results of the trials with them are contained in the following table; where P stands for positive electricity, N for negative, and when it was not strong enough to effect the electrometer, that is denoted by O.

Filings of	Sifted through	Effect.	Results of experiments.
Copper	Copper	O.	
	Tin	N. strong	
	Silver	O.	
	Zinc	P. strong	
	Lead	N.	
Zinc	Copper	P. strong	
	Tin	O.	
	Silver	P. strong	
	Zinc	P. exceeding weak	
	Lead	O.	

—determinable
by varying the
surfaces.

Sieve of one
metal and filings
of another.

ELECTRICITY OF METALS.

Results of ex-
periments.

Filings of	Sifted through	Effect.
Tin	Copper	P. rather strong
	Tin	N. very weak
	Silver	P. strong
	Zinc	O.
	Lead	N. weak
Silver	Copper	O.
	Tin	N. weak
	Silver	O.
	Zinc	N.
Steel	Copper	N. very strong
	Tin	N. ditto
	Silver	N. ditto
	Zinc	N. ditto
	Lead	N. ditto
Lead	Copper	P. very strong
	Tin	O.
	Silver	P. very strong
	Zinc	P. not quite so strong as with
	Lead	O. [silver]
Brass	Copper	P. very strong
	Tin	N. strong
	Silver	P. strong
	Zinc	O.
	Lead	N.
Bismuth	Copper	P. exceeding strong
	Tin	O.
	Silver	P. exceeding strong
	Zinc	P. strong
	Lead	N.
Antimony	Copper	O.
	Tin	N. very strong
	Silver	P. strong
	Zinc	N. strong
	Lead	N. very strong
Nikel	Copper	P. exceeding strong
	Tin	O.
	Silver	P. strong
	Zinc	O.
	Lead	P.

In all the above experiments there was a large surface of contact, and the electrometer only was used; but in those made by Cavallo and others who had a very small surface of contact, electrical signs could not be made to appear without the help of doublers, multipliers, &c. of electricity. I therefore think there can be no doubt about the separating the metals from contact being the cause of their appearing.

If you think the above worthy of a place in your Philosophical Journal, you will very much oblige me by inserting it therein. I am your obedient humble servant,

WM. WILSON.

XII.

Analytical Experiments and Observations on Lac. By CHARLES HATCHETT, Esq. Partly abridged, but chiefly extracted from the *Philosophical Transactions* for 1804, by W. N.

IT is uncertain at what time the useful substance, called Lac, first became known in Europe. It has long been used by the Indians for purposes different from those to which it is applied in Europe. In particular as Mr. Wilkins informed the author, they dissolve shell lac in water by the addition of a little borax, and by adding lamp black or ivory black, they obtain an ink, not easily affected by water, when dry.

The natural history of lac, which is the nidus of the coccus or chermes lacca, has been detailed by Mr. Kerr, Mr. Saunders, and Dr. Roxburgh, in the *Philosophical Transactions* for 1781, 1789, and 1791. It is distinguished into four kinds, of which, however, only three are commonly known in commerce, viz. stick lac, seed lac, and shell lac; the difference of these, with that of the fourth, called lump lac, is as follows.

1. Stick lac, is the substance or comb in its natural state, incrusting small branches or twigs.

2. Seed lac, is said to be only the above, which has been separated from the twigs, and reduced into small fragments; but I suspect it to have undergone some other process, as I have found the best specimens to be very considerably deprived of the colouring matter.*

3. Lump

* Mr. Wilkins informs me that the crude lac, as it is taken from the branches and twigs of the trees, is usually deprived of its colouring

3. Lump lac. 3. Lump lac, is formed from feed lac, liquefied by fire, and formed into cakes. And,

4. Shell lac. 4. Shell lac, according to Mr. Kerr and Mr. Saunders, is prepared from the cells, liquefied, strained, and formed into thin transparent laminæ, in the following manner.

How made from the cells. "Separate the cells from the branches; break them into small pieces; throw them into a tub of water, for one day; wash off the red water; dry the cells, and with them fill a cylindrical tube of cotton cloth, two feet long, and one or two inches in diameter; tie both ends, and turn the bag above a charcoal fire; as the lac liquefies, twist the bag, and, when a sufficient quantity has transfused the pores of the cloth, lay it upon a smooth junk of the plantain tree, and with a strip of the plantain leaf draw it into a thin lamella; take it off while flexible, for in a minute it will be hard and brittle." †

The degree of pressure on the plantain tree, regulates (according to Mr. Saunders) the thickness of the shell; and the quality of the bag determines its fineness and transparency.

Lac is mostly had from Assam. Assam furnishes the greatest quantity of the whole of the lac now in use. ‡

Best quality. Mr. Kerr (speaking of stick lac) observes, that the best lac is of a deep red colour; for, if it is pale and pierced at the top, the value is diminished, because the insects have left their cells, and consequently these can be of no use as a dye or colour, but probably may be better for varnishes.

The feed lac which I have examined, contained but little of the colouring matter, and appeared (as I have already observed) to have undergone some process of purification; but, of all the varieties, shell lac contains the least of the tinging substance, as may well be expected, when the mode of preparing it is considered.

Chemists have paid little attention to lac. It is remarkable, that although lac has been known, and imported into Europe, during so long a time that the date cannot now be ascertained, yet it has but little attracted the attention of chemists.

ing matter by boiling, having been previously reduced, by pounding into small fragments. In Bengal, the silk dyers are the people who thus produce what we call the feed lac, which they do for the sake of the colour.

† Phil. Trans. 1781, p. 378.

‡ Phil. Trans. 1789, p. 109.

The first chemist of eminence who mentions it, and the only one who has subjected it to any thing like a regular examination, is the younger Geoffroy, whose Paper is published in the *Mém. de l'Acad. de Paris* for the year 1714.* Experiments of Geoffroy the younger; In this Paper, Mr. Geoffroy seems to have been chiefly induced to examine it on account of its tinging substance; but he nevertheless has not neglected the substance which constitutes the cells. This he considers to be a sort of wax, very distinct from the nature of gum or resin. But it is to be observed, that he formed this opinion, not so much upon the results of chemical experiments, as upon the cellular construction observed in the stick lac, which, as he justly remarks, demonstrates it to be formed by insects, after the manner that the honeycomb is formed by bees; and that it is not therefore, as some have supposed, a gum or resin, which has exuded from vegetables simply punctured by insects.†

Geoffroy and Lemery obtained from lac, by distillation, — and Lemery. some acid liquor, and a butyraceous substance. Moreover, Geoffroy observes, that when stick lac was thus treated, some ammonia was also obtained, but not when seed lac was employed.

He also mentions another sort of lac, brought from Madagascar, and called by the natives *Lit-in-bitic*. This substance, he says, is scarcely to be distinguished from bees-wax, which much resembles in colour and odour; and that it is produced by a grayish insect, much larger than the *chermes lacca*. It is evident however, from Geoffroy's description, that this substance is very different from the common lac; and there can be little doubt, but that it is the same as that which was, a few years ago, examined by Dr. Pearson, under the name of white lac, a substance resembling the *Pé-la* of the Chinese. ‡

Geoffroy (as I have stated) considered lac as a sort of wax; and since his time it has scarcely, if at all, been subjected to

Notions of chemists concerning lac.

* Observations sur la Gomme Lacque, et sur les autres Matières animales qui fournissent la Teinture de Pourpre. Par M. Geoffroy le jeune. *Mém. de l'Acad.* 1714. p. 121.

† Mr. Kerr observes, that as a red substance is obtained by incision from the *plaso* tree, very analogous to lac, it is probable, that the insects have little trouble in animalizing the sap of these trees, in the formation of their cells. *Phil. Trans.* 1781, p. 377.

‡ *Phil. Trans.* 1794, p. 383.

chemical

chemical examination; it is not therefore surprising, that the opinions of chemists concerning it have been various. Chaptal* adopts the opinion of Geoffroy, and calls it a kind of wax; * but Gren† and Fourcroy‡ regard it as a true resin.

§ I.

Chemical Experiments on Lac. Its Habitudes with Solvents.

Water extracts the colour from lac.

1. Water digested upon powdered stick lac becomes of a deep crimson colour, and takes up about 10 per cent. Seed lac gives out no more than $2\frac{1}{2}$ or 3 per cent. and shell lac only $\frac{1}{2}$ per cent. by similar treatment.

Alcohol dissolves the resin, which is a large part.

2. Alcohol dissolves much of the lac, taking up resin in a clear solution if cold; but if heated, the solution is turbid and scarcely to be rendered transparent, either by repose or filtration.

The resin is obtained either by evaporating the solution or by pouring it into water acidulated with muriatic or acetic acid. The resin which is thus precipitated may be separated by the filter. It amounts to 67 or 68 per cent. in stick lac, and to 88 in the seed lac. Shell lac contains 81 per cent. besides 10 per cent. which is defended from the spirit, and cannot be separated but by subsequent operation. The resin is less brittle than other resin.

Vegetable gluten.

When the shell lac was in small fragments only, these after the first separation of the resin, retained their figure and were more bulky, porous, and elastic. The elasticity was destroyed by boiling water, and the matter itself proved to be of the nature of vegetable gluten.

Ether affects lac very little.

3. Sulphuric ether acts less powerfully on lac than alcohol does. It considerably softens the lac; but produces little other effect.

Sulphuric acid converts it into coal.

4. Concentrated sulphuric acid first acts on the colouring matter; but after a short digestion on the sand bath, the fluid acts on the whole mass, and becomes first reddish brown, and afterwards black, sulphureous acid gas being evolved, and the chief part of the lac being at length separated in a state resembling coal.

* Chaptal's Elements; English edition. Vol. III. p. 387.

† Principles of modern Chemistry. Vol. I. p. 388.

‡ *Système des Connoissances chimiques*. Tome V. p. 624.

5. Nitric acid applied to lac emits nitrous gas at first, and causes the lac to swell much at the same time that it converts it into a deep opaque yellow brittle substance, which by a sufficiency of nitric acid, and continuance of the digestion for 48 hours, is dissolved. The solution however is turbid, and when poured into a large quantity of distilled water, deposits some yellowish flocculi, which, being collected, are found to be a sort of wax. Nitric acid by management and time dissolves it. The solution is turbid and by dilute deposits wax.

The filtrated liquor is of a bright golden yellow; and, when saturated by ammonia, changes to orange colour, but does not yield any precipitate, nor any traces of oxalic or malic acid. The filtered solution contains a yellow matter.

This yellow nitric solution is converted, by evaporation, into a deep yellow substance, which burns like resin, but is soluble in boiling water. Its properties, &c.

The alkalis and lime, being added to this aqueous solution, do not produce any precipitate, but the yellow colour is very considerably deepened; and, by evaporation, an orange-coloured substance is obtained, which is still easily soluble in water, and consists of the deep yellow substance above-mentioned, combined with the alkali or lime.

6. Muriatic acid dissolves the colouring matter and gluten of lac; but its action on these is feeble, unless the resin has been previously separated. 6. Muriatic acid dissolves the colouring matter and gluten.

7. Acetous acid, in its effects, much resembles muriatic acid. 7. Acetous acid.

8. Stick lac, seed lac, and shell lac, are partially dissolved by acetic acid; and, if this be heated, a considerable portion is taken up. 8. Acetic acid acts upon the whole except the wax.

The dissolved part consists of the colouring extract, of resin, and of gluten; the wax being the only ingredient which is insoluble in this menstruum; but a portion of the former substances, being enveloped by the wax, are protected from the action of the acetic acid.

The acetic solution of lac becomes turbid when cold, and deposits part of the resin; a portion however remains in solution, and may be precipitated by water; after which, the liquor retains some gluten and colouring extract, which may be precipitated by saturating the acid with an alkali, and by subsequent boiling.

For the reasons above stated, it would be difficult to effect a complete solution of lac by means of acetic acid; but this may

nevertheless be advantageously employed in analytical operations, when alternately used with alcohol.

Boracic acid scarcely acts on lac.

9. A saturated solution of boracic acid in water, dissolves the colouring extract; but, as the effect does not surpass that of water alone, we may conclude that lac is little, if at all, acted upon by boracic acid.

Borax renders it soluble in water.

10. It has been already stated, that sub-borate of soda or borax has a powerful effect on lac, so as to render it soluble in water; and, as the preceding experiments prove that boracic acid alone scarcely acts upon lac, there is every reason to believe, that the excess of soda present in borax is the active substance; and this conclusion will be confirmed, by the results of subsequent experiments made with the alkalis.

Proportion, &c.

In order to render lac (especially shell lac) soluble in water, about $\frac{1}{2}$ of borax is necessary; and this may be previously dissolved in the water, or may be mixed and added together with the lac.

The best proportion of water to that of lac is 18 or 20 to 1. So that 20 grs. of borax, and four ounces of water, are, upon an average, requisite to dissolve 100 grs. of shell lac; but more water may be occasionally added, to supply the loss caused by evaporation during the digestion, which should be made nearly in a boiling heat.

This solution of shell lac is turbid, and of a reddish-brown colour; when considerably diluted with water and agitated, a weak lather is formed; it is decomposed by acids, and the lac is precipitated in yellow flocculi, which do not apparently differ from the lac originally employed.

The solution is a soap of difficult solubility.

The general properties of the solution shew, that it is a saponaceous compound, which, being used as a varnish or vehicle for colours, becomes (when dry) difficultly soluble in water, although this was the liquid employed to form the solution.

A white thick scum or cream collects on the surface of this liquid, after it has been suffered to remain tranquil for some time, and is found to be produced by a sort of wax, which I shall more particularly notice when the analyses of the varieties of lac are described; but, in the present case, this wax appeared in some degree to be converted into an almost insoluble soap by the alkali of the borax, and may be regarded as the principal cause of the turbidness of the solution.

11. The lixivia of pure soda and of carbonate of soda completely dissolve the different kinds of lac; and these solutions exactly resemble those formed by means of borax, excepting that they are deeper coloured. Soda and its carbonate dissolve lac.

Rather less than $\frac{1}{2}$ of carbonate of soda is required to dissolve shell lac; and this solution, when dried, is sooner affected by damp or water than the solution prepared by borax.

12. Lixivium of pure or caustic potash speedily dissolves the varieties of lac, and forms saponaceous solutions, similar to that in which borax was employed, exclusive of the colour, which is deeper, and more approaching to purple. Also potash,

Lixivium of carbonate of potash extracts a great part of the colouring matter, but does not form so complete a solution of the entire substance of lac, as when pure potash is employed. and its carbonate etc.

The above alkaline solutions, by repose, afford the waxen soap which has been mentioned; and acids, being added to these solutions, and to that formed by borax, precipitate the lac in a flocculent state, and of a yellow or buff colour, which precipitate, when melted, becomes similar to the lac originally employed. If however an alkaline solution of shell lac (prepared, for instance, with soda) be gradually dropped into a sufficient quantity of muriatic acid diluted with an equal portion of water, and nearly heated to the boiling point, and if after boiling the whole for about one hour the coagulum be separated, and the clear liquor be carefully saturated with soda, and again made to boil, a small quantity of a flocculent precipitate is obtained, which was found to be analogous to precipitated vegetable gluten, combined with some of the colouring extract.

13. Pure ammonia, and carbonate of ammonia, readily act upon the colouring matter of lac, but do not completely dissolve the entire substance. Ammonia acts partially.

§ II.

Analytical Experiments on Stick, Seed, and Shell Lac.

Lac, placed on a red hot iron, contracts, melts, smokes much, and leaves a spongy coal. Slow distillation carried to the red heat, gave from stick lac, 1. Water slightly acid, 10; the red heat, gave from stick lac, 1. Water slightly acid, 10; Products of lac by destructive distillation. Stick lac.

2. Thick brown butyraceous oil, 59; 3. Spongy coal, $13\frac{1}{2}$; 4. A small portion of carbonate of ammonia, with carbonic acid, carbonated hydrogen, and hydrogen gas, by estimate $17\frac{1}{4}$.

Seed lac.

Seed lac, by the same process, gave, 1. Acidulated water, 6; 2. Thick oil, 61; 3. Spongy coal, 7; 4. Mixed gas as before, but without ammonia, 26.

Shell lac.

Shell lac gave, 1. Acidulated water, 6; 2. Oil, 65; 3. Spongy coal, $7\frac{1}{2}$; 4. Mixed gas, $21\frac{1}{2}$.

The coal of this last gave one grain of ashes after combustion, which contained a muriate, probably of soda. There was also a little iron, and some particles of sand, probably extraneous.

Analysis of Stick Lac.

Powdered stick lac gave a red colouring extract to water.

A. 200 grains of stick lac, picked and reduced to powder, were digested in a pint and a half of boiling distilled water during 12 hours. The liquor was transparent, and of a beautiful deep red; this was decanted into another vessel; and the operation was repeated, with fresh portions of water, until it ceased to be tinged; the lac then appeared of a pale yellowish-brown colour.

The whole of the aqueous solution being evaporated, left a deep red substance, which possessed the general properties of vegetable extract, and weighed 18 grains.

Alcohol then took up much resin.

B. The dried lac was digested for 48 hours, without heat, in eighteen ounces of alcohol; and the clear tincture being cautiously decanted different portions of alcohol were added, and the digestion was repeated, until the alcohol ceased to produce any effect.

The whole of the solutions in alcohol were then poured into distilled water, which was heated, and an attempt was made to separate the precipitated substance by filtration; but, as this did not succeed, on account of the filter speedily becoming clogged, the whole was subjected to gentle distillation; by which, a brownish-yellow resin was obtained, amounting in weight to 136 grains.

C. The remainder of the lac was again digested in boiling distilled water; by which, 2 grains of the colouring extract were obtained.

Diluted muriatic acid took gluten from the remainder.

D. The residuum was then digested with one ounce of muriatic acid diluted with two ounces of water, which, by boiling,

ing, became of a bright pale red, but changed to purple, when saturated with a solution of carbonate of potash.

A flocculent precipitate was thus obtained, which possessed the characters of precipitated vegetable gluten combined with some of the colouring extract; this, when completely dried, weighed 11 grains.

E. There now remained 25 grains, which evidently consisted of a sort of wax, mixed with small parts of twigs and other extraneous substances. The residue was wax and foreign matter.

A part of the wax was separated by heat and pressure in a piece of linen; and another portion was separated by digestion in olive oil, which assumed the consistency of an unguent.

The residuum was then boiled with lixivium of potash, and became tinged with purple, in consequence of some of the colouring extract which had not been dissolved by the preceding operations.

The undissolved part, now consisting only of the extraneous vegetable and other substances, weighed 13 grains; so that the wax, with a small portion of the colouring extract, may be estimated at 12 grains.

By the above process, 200 grains of stick lac afforded;				Component parts Grs. of stick lac.
A. } Colouring extract	-	-	18	20
C. }	-	-	2	
B. Resin	-	-	-	136
D. Vegetable gluten	-	-	-	11
E. { Wax, with a little colouring extract, about	-	-	-	12
{ Extraneous substances	-	-	-	13
				192.

Analysis of Seed Lac.

200 grains of very pure seed lac were subjected to operations very similar to those which have been described, and afforded, Component parts
of seed lac.

				Grs.
Colouring extract	-	-	-	5
Resin	-	-	-	177
Vegetable gluten	-	-	-	4
Wax	-	-	-	9
				195.

Analysis

Analysis of Shell Lac.

Shell lac boiled
in water gave
little extract.

A. 500 grains of this substance were first treated with boiling distilled water, as above-mentioned, and yielded of extract only 2.50 grains.

Residue gave
much resin to
alcohol.

B. The 497.50 grains which remained, were then digested with different portions of cold alcohol, until this ceased to produce any effect; the resin which was thus separated, amounted to 403.50 grains.

C. As the shell lac had not been reduced into powder, but only into small fragments, these were become white and elastic, and, when dry, were brittle, and of a pale brown colour; the whole then weighed 94 grains.

Muriatic acid
then took up
gluten.

D. These 94 grains were digested in diluted muriatic acid; and the acid, being afterwards saturated with solution of carbonate of potash, afforded a flocculent precipitate, (resembling that obtained from solutions of vegetable gluten,) which, when dry, weighed 5 grains.

Acetic acid, by
long digestion,
took up resin and
wax; the latter
was thrown
down by alcohol;

E. Alcohol acted but feebly on the residuum; it was therefore put into a matrafs, with three ounces of acetic acid, and was suffered to digest without heat during six days, the vessel being at times gently shaken; the acid thus assumed a pale brown colour, and was very turbid. The whole was then added to half a pint of alcohol, and was digested in a sand-bath; by which a brownish tincture was formed, and at the same time a quantity of a whitish flocculent substance was deposited, which, being collected, well washed with alcohol on a filter, and dried, weighed 20 grains.

This substance was white, light, and flaky, and, when rubbed by the nail, it became glossy, like wax; it also easily melted, was absorbed by heated paper, and, when placed on a coal or hot iron, emitted a smoke, the odour of which very much resembled that of wax, or rather spermaceti.

and then the
resin by water,

F. The solution formed by acetic acid and alcohol, being filtrated, was poured into distilled water, which immediately became milky; and, being heated, the greater part of the resin, which had been dissolved assumed a curdy form, and was partly separated by a filter, and partly by distilling off the liquor; this portion of resin amounted to 51 grains.

Carbonate of
potash threw
down some re-
sin,

G. The filtrated liquor, from which this resin had been separated, was saturated with a solution of carbonate of potash;

potash; and, being heated, a second precipitate of gluten was obtained, which, when well dried, weighed 9 grains.

The 500 grains of shell lac thus yielded,

					Component parts of shell lac.
					Grs.
A.	Extract	-	-	-	2.50
B.	} Refin	-	-	-	454.50
C.					
D.	} Vegetable gluten	-	-	-	14.
G.					
E.	Wax	-	-	-	20.
					<hr/> 491.

The mode of analysis adopted for the shell lac, must undoubtedly appear less simple than that which was employed for seed and stick lac: but, upon the whole, it was attended with advantages; for the shell lac being in small fragments, and not in the state of a powder, considerably facilitated the decantation of the solution in alcohol from the residuum; and although, in this last, a portion of the resin was protected from the action of the alcohol, by being enveloped in the gluten and wax, yet, by the assistance of acetic acid, the remainder of the resin, as well as the whole of the gluten, were dissolved; the wax was obtained in a pure state; and a separation of the resin from the gluten was afterwards easily effected, by the method which has been described. As therefore acetic acid is capable of dissolving resin, gluten, and many other of the vegetable principles, it certainly may be regarded as a very useful solvent, in the analysis of bodies appertaining to the vegetable kingdom.

From the results of the preceding analyses it appears, that the different kinds of lac consist of four substances, namely, extract, resin, gluten, and wax, the separate properties of which shall now be more fully considered.

(The Conclusion in our next.)

XIII.

Galvania Illustrations and Remarks. By C. WILKINSON, Esq.

To Mr. NICHOLSON.

DEAR SIR,

Glasgow, Dec. 9, 1804.

On the galvanic
charge, &c.

HAVING, during my galvanic tour, been occasionally informed that the attempt I have made in my *Elements of Galvanism*, to explain the laws of galvanism on the more perfect and imperfect conductors, is not so clear as it was my wish to have rendered it; I shall therefore deem myself much obliged by the insertion of the following observations in your valuable Journal.

The power of a galvanic apparatus, formed of plates not less than $2\frac{1}{4}$ inches in diameter, on animal substances, is directly as the number of plates; on the more perfect conductors, as metals, the power is as you have at large stated in your last Journal.

Let us suppose three galvanic plates: *Fig. 1*, (*Plate II.*) of four inches; *Fig. 2*, of six inches; and *Fig. 3*, of eight inches: An arrangement of these in the form of a galvanic trough, produces equal effects on animal substances, provided the number be equal.

Animal substance is very little superior to water in its conducting power; and the experiments of Mr. Cavendish have proved, that the conducting powers of iron are a million times greater than the conducting powers of water.

A metal brought into contact with another metal disposed to part with a quantity of its compound electricity, will divide the quantity thus disengaged, because its capacity for receiving it is equal to that of the metal under the chemical action.

Animal substance brought into contact with a metal similarly disposed, will only receive a portion adequate to its capacity; and as this capacity is in the ratio of its conducting powers, it will be nearly one million times inferior in capacity to that of a metallic substance; and will, therefore, abstract only so small a proportionate part of electricity to render its intensity equal to that existing in the galvanic plate.

By electrical intensity I understand the quantum of electricity multiplied by the resisting powers of the substance to admit it. Let us suppose the plates 1, 2, and 3, disposed to give out electricity on every assignable point of their respective surfaces. A finger applied at No. 1, *Fig. 1*, will abstract a certain portion of electricity from the points of metal directly in contact; a certain portion of electricity will immediately be determined from the squares 2, 3, 4, 5, 6, &c. from every corresponding square on the surface of the plate, by that motion which Volta has well termed the moto-electrical power: there is a physical resistance in the transmission from these respective squares to the first touched by the finger, and the more remote the greater the resistance: at whatever points this physical resistance shall be equal to the capacity of the substance employed, there all further transmission must cease: this resistance in plates of $2\frac{1}{2}$ inches in the square, appears, from experiment, fully adequate to the constant supply of so imperfect a conductor as animal substances, and therefore any increase of size in the surface of the plate, will produce no additional effects.

When a certain quantity is thus abstracted from the pair of plates at the extremes of the battery, in direct contact with the fingers, these plates and the interposed fluid in the cells, only act as conductors to the next pair, and so on; as the velocity of discharges of each successive pair is so infinitely greater than the time required for the chemical action, is the reason why the effects we experience are increased in proportion to the numbers; if we were to suppose that the time required for the successive discharge of 50 pair of plates, should be precisely equal to the time required for the chemical action, we should derive no increase of power by an increase of number, which is contrary to experience.

I have a battery formed of 600 plates, each plate only exposing half a square inch in surface: To experience equal effects from this arrangement, I am obliged to pause three or four minutes between each contact; an interval of time no ways requisite in larger plates.

When such superior conductors as metals are employed, the physical resistance of transmission from one part of the plate to another, bearing no proportion with the capacity of the

the metal, every assignable point of the galvanic plate is efficient at the same time, therefore the intensity will be as the surface, and the powers as the squares of the intensities.

I am, Dear Sir,

Your's, respectfully,

C. WILKINSON.

No. 19, Soho Square.

XIV.

*On the Devitrification of Glass and the Phenomena which happen during its Crystallization. By DARTIGUES.**

Glass is not a crystallization.

SOME philosophers have considered glass as a crystallization; and this opinion appears natural on considering the transference of glass or of crystal, from the latter of which the term for designating the regular and spontaneous arrangement of the particles of bodies has been taken. But on closer reflection, this notion is found to be erroneous. In fact glass never affects a crystalline figure, either in its surface or its fracture; it never presents crystals of its own proper substance, such as are seen in metals slowly congealed; and whenever crystals are formed in the mass of glass, they are foreign to the part which still continues vitrified; they may be considered as a step the reverse from that of vitrification, as I shall show in the course of the present paper.

Vitreous fusion distinct from that of salts.

On the first explanation of vitreous fusion, I must distinguish and separate it from the fusion of certain other bodies which flow without addition in the heat of our furnaces, such as borax, the phosphoric acid and others. In these bodies the con-

* Translated from the *Journal de Physique*, LIX. 5.

The author of this memoir, who is proprietor of the glass-works and other establishments of Voneche (Sambre-et Meuse) has undertaken to draw up for the French National Institute a treatise on the art of glass-making, to serve as a continuation to the arts and trades of the Academy. The first part is in readiness to appear, many of the plates being ready. The second part relates to the uses of glass in the arts. And the third part consists of detached memoirs upon the physical and chemical properties of glass. The present dissertation is extracted from one of those memoirs.—J. C. Delametherie,

denied

denser heat softens and renders them fluid, and they more or less continue to exhibit, when cooled, the transparency and some other of the well known physical properties of glass.

But in the examination and description of that fusion to which vitreous compounds are subjected in order to fit them for the purposes of life, it is necessary to observe that the effect arises from two phenomena. It is not simply the result of accumulated heat, but it is jointly produced by the affinities of the substances which enter into the mixture. These substances which tend to combine and mutually to penetrate each other, act according to the laws of their affinities as soon as ever the temperature is sufficiently elevated. Thus it is that a mixture of several earths becomes fused at a temperature which could not have rendered any one of them fluid. It is a solution by heat.

The common vitrification is therefore the result of a combination effected at an elevated temperature between different and heterogenous substances; and the product is a compound perfectly homogenous more or less transparent, elastic, and breaking in a peculiar manner, from which the term vitreous fracture is derived. This body is a remarkably bad conductor of heat and of electricity; it is capable of becoming soft at a temperature inferior to that at which it was fused, so that it may be rendered pasty, ductile, &c. Nature of the compound; or glass.

The phenomenon during which all these properties disappear is what I have called devitrification; an expression which may at first seem extraordinary, but which the facts will shew to be perfectly accurate. Devitrification.

Several philosophers have before noticed this effect; some have even made observations upon it, and have noted various circumstances scarcely connected with each other; but I am not aware that any one has yet published a series of researches proper to explain the effects, so as to shew that it is one of the known properties of all natural bodies, being in fact nothing more than the product of a crystallization. It is the consequence of crystallization.

Reaumur first observed that a glass, more especially if it be composed of different earths, as bottle glass is in general, may be decomposed, and lose its transparency and other vitreous properties. Fully occupied in his researches upon porcelains, he was desirous of applying this discovery to the fabrication of potteries, and attributed the phenomenon to the substances in which he had cemented his glass. The fact was The porcelain of Reaumur:

denominated the cementation of glass, and the product was called Reaumur's porcelain: no incident could be more effectually calculated to retard the true developement of the fact than such a denomination of this kind.

Repetitions of the process by d'Antic and others.

The labours of Bosc d'Antic upon the same object were also directed to no other purpose than that of obtaining a good pottery by this means, and of ascertaining what cements were the best calculated to give new properties to this body. Thus it was that by calling the process by the name of cementation, which depended in no respect upon what was added as the cement, others were misled, who were induced to follow the course of experiments already began. The natural consequence was, that science gained nothing respecting this process since the time of Reaumur. Many have since attempted the cementation of glass without perceiving any new result in the product.

Observation of crystals in glass.

Several persons have since admitted the property in glass of affording crystals; but these remarks being more especially made by artists, placed by their situation at the head of glass-works, have not afforded the scientific consequences which might have been deduced from them. The directors of a great establishment have seldom time to dwell upon the contemplation of small effects; they are obliged to attend to too many things at the same time. These remarks, though curious in themselves, remained without connection; and no one thought or ventured to publish that the crystallization of glass, and cementation by the process of Reaumur are absolutely one and the same thing.

Experiments of Sir James Hall on whinstone, &c.

Sir James Hall,* in his valuable experiments upon whinstone and lava, published in the 14th volume of the *Bibliothèque Britannique*, ascertained the property of these stones to become fused into glass, and to return to the stony state, according to the circumstances.

He did not pursue this devitrification among artificial products.

He called this last fact a devitrification. He saw that it was the effect of a precipitation, and explained it in a manner that was highly satisfactory and true; but being too much occupied in deducing from this fact arguments in favour of the volcanic geology, he neglected pursuing the interesting philosophical consequences to which the phenomenon pointed the way. This is

* First in the *Edinburgh Transactions*, and afterwards in our *Journal*. N.

the task I have undertaken, and in the present memoir on devitrification, I shall give the results of my first experiments. As my situation gives me the use of a fire of extreme violence and continued for years together, it may have been in my power to make observations not within the reach of every one. The facts I shall report do partly explain themselves; they are the result of laws to which all bodies are subjected. All the merit of the observation consists in having seen them in substances, and on occasions where it was not known that these laws do take place.

The bottom of the furnaces for fusing or fusing the glass usually presents large cavities excavated by the action of the fire, and of the corrosive substances which often flow out of the melting pots. These cavities are filled with a kind of glass called picadil, which is composed of ashes which become vitrified, stones of the furnace which become fused, and more particularly of glass which falls from the pots. Care is taken to remove this at each font or making of glass. When the furnace is nearly worn out, the cavities having become large, cannot be entirely cleansed, but some of the picadil remains. When the furnace is extinguished, this picadil undergoes an extremely slow cooling, because it is surrounded with masonry of several cubic fathoms, which is itself penetrated with heat that has continued for upwards of a year. I have always remarked that it was in the glass of these bottoms of the furnace that I found crystallizations through the mass of glass, the rest of which was very transparent and pure. These crystallizations, which were always considerably regular and numerous, excited my curiosity, as they had done that of other glass-makers before me. I collected many specimens, taking care to choose those which appeared the most curious, and presented the most extraordinary characters.

It is very remarkably seen in glass furnaces.

Crystals in glass.

Soon afterwards, from comparing the pieces themselves, and the circumstances under which they had been produced; and by the combinations of remarks, trials, and experiments to imitate these crystallizations at pleasure, I at last succeeded in distinguishing various classes, all of which are produced by the nature of the different circumstances which accompany the formation of the glass. I shall give a rapid outline of these, without entering into any remarks on the devitrification which almost constantly takes place in the scoria of forge furnaces.

Examination of the facts.

This

This last fact must have presented itself to every one, and will without difficulty be referred to the observations I shall make.

The more compounded the glass the more easily it loses its vitreous nature.

The first remark which offers itself is, that the more compounded the nature of the glass, the more easily and readily it will become devitrified: but in the same manner as a solvent loaded with a great number of saline substances of different kinds, suffers them to crystallize more confusedly, so likewise it is found that these glasses do not present the most regular crystallizations. A precipitation takes place through the whole of the mass; each of the component parts obeys the laws of affinities; at the same time the transparency speedily disappears, and we no longer see a piece of glass, but a stone. Through this confused mass it is nevertheless impossible not to observe the rudiments of crystals. Such is the manner in which bottle glass comports itself in its devitrification. These approach very much to the state of mere earthy glasses, having very little salt in their composition.

Bottle glass.

Experiment in the small way.

Every one has an opportunity of keeping a common bottle of black glass in a fire long continued, and capable of softening its paste; it speedily changes colour, becomes grey, and assumes the appearance of stone ware. In this manner we form the pottery of Reaumur, but without any process resembling crystallization.

Particular examination of the effect by slow cooling.

But instead of observing the phenomenon in so small a mass, if we examine the bottoms of the glass furnaces in which these bottles are fused, we find that the glass is absolutely devitrified, and has assumed an appearance so completely stony, that the most experienced eye can scarcely distinguish the bricks of the furnace from the part which has been glass. It is only by following the course of the devitrification in pieces less advanced, that we succeed in distinguishing the glass in a granulated stone, which has rather the appearance of coarse pottery, or a strongly baked clay.

Instance of the effect taking place in a short time.

It often happens that the slow cooling of an hour or two is sufficient to produce the entire devitrification of bottle glass. I have pieces of eight centimetres ($2\frac{1}{2}$ inches) thick, which I obtained at the glass works of Mr. Saget, of La Gare. One of the pots was taken out of the furnace to be replaced; the glass remaining at the bottom of this pot was preserved from cooling during the time the pot itself acquired the common temperature, and by this means the nature of the glass was entirely

entirely changed. It no longer exhibits the transparency, but is a mass of crystals, composed of small needles, converging towards common centers. It has no longer the appearance of glass.

This fact shews with what facility bottle glass is devitrified, and always without the least appearance of cementation. The infinite variety of substances which make up the composition of bottle glass, greatly modifies the phenomena which take place during their devitrification, and must, no doubt, influence the form of the crystals; but my opportunities of observing this kind of glass have not been numerous.

Passing therefore to glasses less earthy and composed of a less number of substances, when I make the same examination of the bottoms of the furnaces of glass for windows called glasses of Alsatia, or half clear glasses, into which is put a greater portion of pure sand and of alkali, I observe nearly the same phenomenon; but as they are less sudden, they are more easy to distinguish and to separate. At the first instant, and in the masses where the devitrification commences, we seem to observe a small portion of blue colour diffused through a greenish liquid.

Greenish glass of Alsatia is less compounded, and changes more slowly. First change, a bluish colour.

A very singular fact here presents itself, which I shall here only point out, with the intention of more fully examining it hereafter. This greenish glass mixed with blue, appears, in fact, to become of an obscure blue when considered by reflected light; but if placed so as to transmit the light, it always appears greenish; the blue being reflected and the green refracted, each singly.

By continuing to observe the devitrification of this glass, it is soon perceived that the blue precipitation is followed by another more abundant, which affords a dirty white, and is very distinct from the former. The colour of this glass becomes deeper and deeper, and at length it resembles grey horn.

Second change, dull white opacity.

In these different transitions the paste of the glass appears constantly to remain, distinguishable in its polish, its fracture, and all its other properties, except its transparency. But in the midst of this paste resembling horn, very distinct crystallizations are formed, consisting of nodules composed of small needles, all converging towards the center. In this state it is no longer glass, but a crystal possessing all the physical properties of mineral substances left to themselves.

Third change, needled crystals in groupes.

DEVITRIFICATION OF GLASS.

An exact analysis made of a certain number of these crystals carefully detached from the mass, might indicate their nature and throw light on their formation.

It often happens that these crystallized nodules are enveloped in a crust which seems foreign to their nature, and may be compared to the crust which envelopes flints in the middle of the chalk-banks in which they seem to grow.

Speedy refrigeration confuses the phenomenon.

These are the series of phenomenon attending the devitrification of greenish glass when it happens slowly; but if the effect is too rapidly produced, the appearances resemble those observed in the devitrification of bottle glass. The window glass here spoken of, is of that kind which contains very little of earthy matter, except wood ashes. It must admit of many variations according to the differences in its composition.

Clear and simple glasses are not easily devitrified.

White or colourless glasses are very difficultly crystallized or devitrified. When they are well made, we may affirm that a long continued heat does not change them; but for this property it is necessary that they should be composed merely of silice, with the precise quantity of flux for its saturation. In this case, a very long continued fire will no otherwise change them than by rendering them more yellow and hard.

(The Conclusion in our next.)

ADVERTISEMENT.

Various articles of *Philosophical News*, *Accounts of Books*, and other subjects, have necessarily been postponed this month, on account of illness with which the Editor has been afflicted.

The same cause has also unfortunately prevented his Draughtsman from completing some Designs, which would else have appeared.

Mr. Ezekiel Walker has been prevented, by severe indisposition, from answering the paper of C. L. in our last, upon the *Horizontal Moon*.

A note has also been received from C. L. in which he begs leave to notice, that the large lens mentioned in his experiment I. has a clear aperture of 6,1 inches in its frame, and was so used in that experiment.

*Egyptian Engraving on Porphyry
probably astronomical.*



Galvanic Illustrations.

Fig. 1.

3	4		
1	2		
5	6		

3	4				
1	2				
5	6				

Fig. 2.

3	4					
1	2					
5	6					

Fig. 3.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,

THE ARTS.

FEBRUARY, 1805.

ARTICLE I.

*Experiments and Remarks on the Augmentation of Sounds. In a
Letter from Mr. JOHN GOUGH.*

To Mr. NICHOLSON.

SIR,

A FACT is mentioned in a former communication of mine ^{Sounds heard} to your Journal (octavo, vol. III. p. 41) which proves, that ^{further from an} a stroke given to an extensive vibrating surface by a slender face, ^{extended sur-} rod, produces a sound possessing a high degree of force: from this it follows, that the range of a sound may be extended by enlarging the vibrating surface, while the magnitude of the impulse remains the same. The preceding maxim is of great use in acoustics; but as the truth of this proposition rests at present on the authority of a local observation, it may not appear superfluous to give a few easy experiments in confirmation of the fact.

Experiment I.—My first attempt endeavoured to prove, that Exp. 1. The, when a sound ceases to be audible from remoteness, the en-^{beating of a} largement of the vibrating surface will again render it distinct, ^{watch audible at} at the same distance. For this purpose, a watch was sus-^{a greater distance} pended, on a calm day, from the branch of a tree, about $5\frac{1}{2}$ ^{when in contact} feet from the ground. The clicking of it barely reached my ^{with a metallic} ear.

ear, in those intervals of silence which were not disturbed by foreign sounds, at the distance of $5\frac{1}{2}$ yards; but when I removed half a yard farther, the strokes of the balance were no longer heard. A circular plate of rolled iron, one foot in diameter, was then brought into contact with the part of the watch farthest from me, the position of it being such as to present the plane of the circle to my ear. This circumstance increased the range of the sound; for every stroke of the instrument was distinctly perceived at the distance of four yards, when the silence of the place remained undisturbed by other causes. The result of this experiment is too obvious to need a comment: and I have only to add, that when the trial is made within doors, the range of the clicking is greatly increased; because the primary sound receives an accession of force from a number of its own pulses, which are reflected to the ear by the furniture and walls.

Exp. 2. The plate gives a considerable sound itself.

Experiment II.—I proceeded, in the next place, to examine the effects produced on the auditory organs by the vibrations communicated to an elastic surface by a watch; the immediate sound of which was smothered by art. For this purpose, a watch was placed upon a cushion, under an inverted porcelain cup, which was wrapped externally in several folds of flannel. The instrument was heard with difficulty at the distance of one foot in this sort of confinement; but when I placed the muffler with the watch under it, upon a square mahogany table four feet broad, the clicking noise imparted by it to the top of the table, reached the ear very distinctly at the distance of four yards. The same watch, covered in like manner upon the iron plate mentioned above, reached my ear at the distance of 22 feet. The apparatus stood in this trial upon a round oak table, 24 inches in diameter.

The enlargement of the surface augments the sound.

The preceding fact proves indisputably, that the secondary vibrations of an elastic surface actually affect the ear, in those cases wherein the pulses never reach the person of the hearers, which proceed from the part that is exclusively considered as the seat of sound; in other words it proves, that the cause of a sound is not confined to the obstacle receiving the primary impulse, but that it is propagated from the point of impact through the contiguous bodies. The first experiment also shews, that the enlargement of the vibrating surface increases the force, which a given stroke exerts upon an ear placed at

a given distance; consequently the increased force thus imparted to a sound, must be ascribed to the co-operation of the vibrations communicated in the first experiment to the plate of iron; for by uniting with those of the watch, they evidently augmented the power of the latter instrument.

A rational theory of the forces of sounds may be formed, by help of the preceding observations, in the following manner: *Theory of the force of sound augmented by the vibrations of a surface touching the same body.* *First*, Suppose the plane ABC (*Plate IV.*) to be one of the surfaces of an elastic body, and let it receive an impulse at the point O ; then O will recede from the stroke in a right line perpendicular to ABC , drawing after it all the contiguous particles, which, in their turn, will change the place of the next circle of particles. In this manner a circular dent $OMEQTF$ will be formed; the center of which O will be below the plane ABC ; M , the highest part of the margin, will be above it; and the extreme circle $EQTF$, bounding the whole, will lie in the plane. *Second*, Thus will a circular swell be formed on the plane, resembling the circular wave produced by a stone dropped into water. A series of these swells will follow the first, each of which, in succession, will lie more remote from the center O than its predecessor. *Third*, The collective force of each swell is equal to that of the first impulse; and it is distributed over the surface of a ring, having O for its center and OE for its breadth; *i. e.* all the rings are of equal breadths, but unequal diameter. *Fourth*, The effect of the stroke is thus progressively propagated from O to the more distant parts of the plane ABC , with an uniform velocity. *Fifth*, Let P be the place of the ear in the right line OP , perpendicular to ABC ; also let $GHIJK$ be any swell in the same plane; then will the effect of that swell be carried to the ear, in the conical shell $OGIJKP$; and the impulse imparted to the auditory organs, through the medium of this shell, will be greatest when the diameter of the ring OI is least, and the contrary. *Sixth*, If ABC be a concave spherical surface, having P for its center, all the swells will successively exert equal forces on the ear at P . *Seventh*, If OP be put equal to a , and f measure the force of a stroke at the distance l ; then let v equal the velocity with which this force is propagated from O over the spherical surface, and t the breadth of the rings covered by the swells, the force impressed on the ear at P in a given interval of time, is equal

to $\frac{fv}{la^2}$. *Eighth*, Though time is infinitely divisible in a mathematical sense, M. Euler has shewn *, that the ear conceives it to consist of indivisible or elementary particles; consequently all the forces which arrive at P during one of these elementary intervals, make a single indivisible impression on an ear placed at that point; because this organ cannot take cognizance of a smaller particle of time; therefore the force of a sound, produced at the center of a hollow sphere by the undulations of its surface, is truly denoted by the expression $\frac{fv}{la^2}$. *Ninth*, In general, the force of a sound, at any instant of its duration, is equal to the sum of the forces exerted by those pulses, which strike the ear in the elementary interval: Consequently, if the distance between the hearer and the sounding body be such, that lines drawn from his ear to every point of the vibrating surface, may be considered as equal, the force of the sound will be expressed by $\frac{fv}{la^2}$. *Tenth*,

To shew the nature of this theorem by an example; suppose a bell A, the diameter of whose mouth = $1\frac{1}{2}$, to be an octave below a bell B, the diameter of which = 1: also let A be heard twice as far as B, when each receives an equal stroke; the ratio of the breadths of the swells in A and B is required. Let V, v , be the velocities of the swells in A and B, L, l , the breadths of these swells; also let 2 and 1 be their respective ranges: then since f is given, $\frac{V}{4L} = \frac{v}{l}$, by the theorem; hence, as $V : 4v :: L : l$. Now the semi-circumferences of A and B are in the ratio of $1\frac{1}{2}$ to 1; but while the vibrations pass over half the circumference of A, they pass and repass over the same part of B: therefore, as $V : v :: 1\frac{1}{2} : 2$: hence also, as $L : l :: 1 : 6$. Q. E. I.

The principle
extended to
voices and other
sounds.

Should the principle explained above be admitted, the following conclusion must also be received as a necessary consequence of it: the voices of animals, as well as the notes of musical instruments, and the reports produced by blows given to less elastic substances, derive no inconsiderable portion of their respective forces from the vibrations of parts which are

* Tentamen de Mus. Cap. I. Sec. 13.

not directly concerned in the production of the primary sound. This opinion appears to be countenanced by an experiment, which I have repeated at different times under various forms, and of which the following is the substance: If a wire be stretched by two pins fixed into a bad conductor, such as a block of stone; the sound produced by it is much weaker than that of an equal wire similarly stretched upon a board, which is a better conductor. In like manner, if a circular piece of wood be struck by a leaden ball constantly falling from the same height, the report will be heard at a greater distance, when the wood is placed upon a good conductor of sound, than when it rests upon a bad one. These facts create a high degree of probability, that the leading maxim of this essay is applicable to sounds of every description, embracing such as are continuous, as well as those of a momentary duration.

Dr. Matthew Young's Enquiry into the Phenomena of Sounds has fallen into my hands since the foregoing remarks were committed to paper, and a partial perusal of the work has convinced me, that the present theory has not all the claim to originality which I once supposed to be due to it. The justice and necessity of the preceding observation will appear from the Doctor's two-theories of the speaking-trumpet and echoes; in both of which he has made use of my leading principle with success. The maxim, however, has been extended to a greater number of cases by these observations; besides which, an attempt has been made to demonstrate the truth of it by experiment, and it is on the two circumstances here stated that the merits of the present letter must rest; for I am far from desiring to rob the celebrated author of the enquiry of the honour due to his sagacity.

JOHN GOUGH.

Middlesex, Jan. 4, 1805.

II.

Observations on the different Degrees of Facility with which Masses of the same Material admit of Changes in their Temperature; with Applications of the Facts to the Construction of Pendulums, and Speculations upon various new Forms of pendulous Regulators of Time. In a Letter from J. WHITLEY BOSWELL, Esq.

To Mr. NICHOLSON.

SIR,

Compound pen-
dulum.

THE satisfactory communication on an ingenious improvement of the compound pendulum, in your Journal for December, has reminded me of some ideas which occurred to me on similar subjects. In hopes that they may be of some use in a matter so interesting in itself, and so important in its application, I send them for your opinion.

A very material point seems to have been omitted in all the considerations I have met with on the expansion of bodies by heat; pyrometrical experiments being directed to that merely of different substances of the same size, but none being made on bodies differing in bulk.

Bodies are more
or less speedily
expanded by heat
according to
their dimensions
and figure.

Though various bodies differ in their degree of expansion by heat according to their materials, yet all are subject to certain laws on this point, depending on their dimensions; for as bodies receive or communicate heat by their surfaces, and retain it in proportion to their bulk, it follows, that their mutability of temperature must depend on the ratio of their solid contents to their surfaces; and that the greater the surface in proportion to the bulk, the more readily will a body change its temperature; and on the contrary, the greater the bulk in proportion to the surface, the less will it be affected by the fluctuating heat of the surrounding medium.

Reduction of the
effects.

The proportion which the surfaces of bodies bear to their bulk may be varied, either by altering their shape or changing their size. In the first respect it is sufficient to note, that the flatter and longer any body is, the greater will be its surface in proportion to its mass of matter, and *vice versa*: the difference caused by the variation of bulk can be more easily calculated, as in bodies of similar figures their surfaces are
exactly

exactly as the squares, and their solid contents as the cubes of their diameters. From this, and what has been already premised, it will follow, that in bodies of different sizes, similarly shaped, and of the same substance, the capacity for heat will be as the cubes, and the mutability of temperature as the squares of their diameters or sides; and that, therefore, the degree of the tendency of those bodies to maintain an equality of temperature, may be estimated as their solid contents *minus* the relative value of their surfaces: Bodies whose shapes are dissimilar, will in some degree be subject to the same rule; but it is not material to the present subject to take them farther into consideration.

The following table of the proportion of four cubes in the Table of the rapidity of heating and cooling.
above respects, whose sides are reciprocally as 1, 2, 3, and 4, will exemplify what has been above asserted, and shew how greatly the retention of temperature of bodies is increased by adding to their bulk.

	Side.	Surface.	Change of Heat as	Bulk.	Capacity for Heat.	Retention of Heat estimated.
	Feet.	Square Feet.		Cubic Ft.		
A	1	6 : 1	1	1	1	1
B	2	24 : 4	4	8	8	4
C	3	54 : 9	9	27	27	18
D	4	96 : 16	16	64	64	48

From these considerations the following inferences may be drawn: 1st, That the greater the bulk of any body, the less will be its mutability of temperature in proportion, and of course the less will it alter its degree of expansion: 2^d, That a large globe in the first place, or a cylinder, whose height was equal to its diameter, in the next place, or in the third place a large cube, would have its dimensions very little changed by the fluctuations of atmospherical temperature.

To apply these principles to the regulation of horological movements may appear difficult, as the first idea that would occur is, that it would be necessary to put those large bodies in motion for this purpose; but this is by no means needful, and it may be effectually performed by constructing and fixing up a pendulum in such a manner, that its variations in length shall be corrected by, and depend on, those of the large body.

There

Application of these facts to clocks.

Pendulums of
Crosthwaite and
Pine.

There is no need of much investigation to discover the mode of doing this; as either the pendulum invented by Mr. Crosthwaite, of Dublin, or that contrived by Mr. Pine, will fully answer this purpose, though they will by no means perform that for which they were intended,--of effecting a compensation of themselves, in the manner of the gridiron pendulum.

Their mistake.

The error of these gentlemen, in this respect, is very fully pointed out in a paper signed A. B. published in the seventh Volume of the Repertory of Arts, which would have been much more creditable to its author, had he not triumphed so much in his superior penetration on the occasion, which has led him to forget himself so far as to descend to the illiberality of national reflection on the part of Mr. Crosthwaite: for which reason I own I am happy to have it in my power to give these gentlemen the satisfaction of seeing an error pointed out in his paper in return; which is, where he asserts that a small rod of mahogany must have the same contraction and dilation, from change of heat, as a *solid plank* of the same wood, or words to that effect, the fallacy of which opinion appears from what has been here laid down: It would be easy to distinguish this philosophy by an appellation of the same stamp as that which A. B. has bestowed on Mr. Crosthwaite's invention, and the more unfortunately for his insinuation on this occasion, as the Irish method happens to be the best of the two; for Mr. Crosthwaite supported his pendulum by a solid wall, which would be much less affected by change of heat in the air than the back of the clock-case to which Mr. Pine attached his.

Use of a large
block of stone
for fixing a pen-
dulum and clock.

The best method of applying the foregoing facts to the regulation of clocks appears to me to be, to procure a large cylinder, or octagonal prism, of stone, the diameter of whose base is equal to its height, and exceeds the length of the pendulum by some inches at least, and as much larger as it could be got the better: one of a cubic shape would also do, if its mass of matter exceeded that which could be got of the other forms; for this purpose granite stone seems preferable, and in the next place Portland stone, as they can be easily had in large blocks; but it is probable that stones which grow damp in moist weather would not be so proper for this purpose. To this block of stone a pendulum should be attached with a single compensating

compensating rod added to it, in the same manner as that constructed by Mr. Crosthwaite or by Mr. Pine: the clock itself should also be fastened to the stone, particularly if Mr. Crosthwaite's pendulum is used.

The block of stone may be farther secured from change of temperature, by being surrounded on every side by brick-work; except where the pendulum and clock are fixed, and having dry saw-dust rammed in between it and the brick-work. It is probable that a block thus fitted up, would vary little from changes in the heat of the air; and in those situations where large blocks of stone are found naturally, this might be done at a small expence: where a large block of stone could not be procured, a piece of dry straight-grained red deal balk, or solid mahogany, might be used to good effect, if well painted, and inclosed as before directed: and for common use, it is probable that a large glass tube, filled with semen lycopodii, if to be had, or fine dry saw-dust, well closed at either end, and covered with oiled silk, would form a pendulum-rod little affected by heat or cold; but if metal is preferred, then a tube of metal, fitted up as last directed, would be preferable to the small wires now used for this purpose.

The advantages to be expected from the principal method above-mentioned, of fitting up pendulums, is, that it affords an unlimited mode of approximation to perfect compensation; and that, as it requires no great refinement of workmanship in its construction, it can be used in many situations where it would be impossible to have a tubular compensating, or common gridiron pendulum made; which, notwithstanding the late improvements, are extremely difficult to construct with accuracy, as may be seen in the paper published in your Journal for December last, where many of the impediments to their perfection are pointed out, and more still remain to be noticed, of which the following deserve some attention.

The metals of which gridiron pendulums are composed, are both of them mixed metals, and of course every different parcel of them made, must vary in some degree in the relative quantities of their component ingredients, and from thence in their degree of expansion by heat. It may appear strange to call steel a mixed metal; but when it is considered that charcoal forms a considerable part of it, the justice of estimating it as such will be evident; and in this metal the proportion

Defence of the
block from
change of tem-
perature, &c.

Advantages.

Imperfections of
compound pen-
dulum stated.
The metals are
mixed, and not
constant in their
composition.

portion of charcoal in different parcels of it, is much more various than that of zinc is in brass: perhaps, for this reason, it might be better to use some of the simple metals in the place of these, in their construction.

Difficulty of adjustment.

Another difficulty in forming these pendulums with accuracy, arises from the want of a good method of proving them. The adjustment by the going of a clock is shewn to be imperfect in the paper on the tubular pendulum, and that proposed in its place, the use of the pyrometer, is equally defective, for the following reasons:

Defects alledged in pyrometers.

Pyrometers are unfit for measuring the effects of the heat and cold of the atmosphere on any substance, because their own machinery, and particularly the part supporting the substance under trial, is liable to be affected by the same causes: and in this case the index will shew the sum of the alteration of the substance, the part of the apparatus between its two extremities, and the machinery of the pyrometer, instead of that of the substance alone: and in applying artificial heat to the matter under trial, it is extremely difficult to communicate that heat equally to all its parts at the same time, and so as not to operate on the pyrometer itself, without which the exactness of a compound pendulum could not justly be tried by it.

Lastly, another error is produced in the computation of the aberration of all pendulums, from not taking into account the dilation and contraction of the small steel spring by which they are usually suspended.

Other pendulums.

As all the oscillatory pendulums yet made public are influenced in their notation of time by the expansion of their substance, and as the best contrivances to compensate this are only an approach to perfection, an increase of probability but no certainty, it is therefore an object worthy attention to investigate other methods for effecting the same purpose: for which reason I shall here beg leave to suggest the reconsideration of a species of pendulum of a different nature, which has never been condemned on any just ground that I could hear, and which in fact is so little known, that the application of a similar movement to other purposes, is by many supposed to be a late invention. The pendulum which I allude to, is that treated of in the fifth Part of the *Horologium Oscillatorium*, of the well known Huygens, published in 1673, which he calls

calls the circular pendulum, and which much resembles the Circular pendulum of Huygens. centrifugal apparatus, used frequently about steam-engines to regulate the aperture of the steam-damper, of which it probably suggested the first idea. To form the circular pendulum, a spindle (H D, Fig. 1, Plate III.) proceeds perpendicularly from the clock-work, from whence it receives a circular motion round its own axis; to this is affixed a lamina (B G A) of some breadth, bent according to a paraboloidal line, by the evolution of which (after it is joined to a certain right line) a parabola is formed, the construction of which is shewn in the eighth proposition of the third part of the above work: This lamina causes the ball of the pendulum (F), attached by two threads to its upper extremity (as it circulates), to perform all its revolutions (which will be of greater or less extent as the axis moves with greater or less force) in the surface of a conoidal parabola (F E). Huygens declares, that all the circles performed by the pendulum thus constructed, will be isynchronous, and then shews how to proportion the parts of the apparatus, so as that each of its revolutions shall be performed in a second, or in half a second: He says, the only reason which caused the oscillatory pendulum to be preferred to this was, that this last was more difficult to construct. As this circular pendulum seems to possess the valuable property of correcting the alterations caused in it by expansion, or of rendering them of no consequence, the difficulty of construction is a matter of no consequence, especially as there are now to be found artists so much more excellent in works of this kind than formerly.

Huygens gives the following character of this pendulum, which I transcribe in his own words: "*Plura tamen, hujus quoque generis de quo nunc loquimur, nec sine successu, constructa fuisse: estque in his singulare illud, quod continuo atque æquali motu circumferri cernitur index præstremus, qui secunda scrupula designat, cum in omnibus aliis horologiis subsistim quasi feratur: Item hoc quoque, quod absque strepitu sonoque omni moveantur hoc ratione constructa automata.*" He concludes the book with thirteen theorems, *De vi centrifuga ex motu circulari*, several of which prove this kind of pendulum to have most valuable qualities: the sixth of these, being very remarkable, I copy for your readers, who may not have an opportunity of seeing the work which contains it, as follows:

" THEOREMA.

“ THEOREMA.

“ In cava superficie conoidis parabolici, quod axem ad perpendicularum erectum habeat, circuitus annes mobilis, circumferentias horizonti parallelas percurrentis, five parvæ five magnæ fuerint; æqualibus temporibus peraguntur: quæ tempora singula aquantur binis oscillationibus penduli, cujus longitudo sit dimidium lateris recti parabolæ genitricis.”

From the above theorem, and what has been already laid down on the subject, it is evident that the circular pendulum is well worthy of a fair trial, and should be recommended to the attention of all scientific constructors of horological movements.

As this circular pendulum will take up more room than a common one, when this is any considerable inconvenience, one of the following constructions on the same principles (which have occurred to me while writing this communication), may be used in its place.

Various constructions of the circular pendulum.

The first is as follows: To a short spindle ascending from the clock, as before described, let there be attached a glass or iron tube, bent in the form of the conoidal parabola before-mentioned, and placed as E F in Fig. 1: into this tube let there be poured a sufficient quantity of mercury to serve as a centrifugal weight.

It appears to me that the mercury rising in the tube, as it circulates, by the centrifugal force, along its parabolical curve, will have the same effect as the weight in Huygens's construction, caused by other means to move in a similar line. The tube should be sufficiently large to admit the air to pass freely above the mercury as it moves along its cavity, or else a small aperture should be made in the upper surface of the tube near the spindle, for the same purpose: to prevent also the error which might otherwise be caused by a varied resistance to the motion of the circulating tube from the mutability of atmospherical pressure, the tube may be inclosed in a circular vessel covered at top, made of as light materials as possible, and very smooth externally, through whose axis the spindle should pass, and to which it should be united so as to revolve with the tube. A second method of effecting the same purpose is, to have a semi-cylindrical trough shaped and placed in same manner as the above tube, in which should be put a metallic sphere of smaller diameter than the trough, that the sphere

sphere might move in it without friction; for, being thus formed, it follows, from the 13th of the third book of Euclid, that the sphere could never touch the trough but in one point alone at the same time. A third method of construction is, to place a cylindrical metallic rod, or thick wire, bent into the parabolical curve before directed, in the same position as the above tube and trough; with a spherical weight put on it so as to move freely along it, which last might be effected by a friction-roller, either let into the upper part of the weight, or placed above it, whose surface should be formed into a circular groove of larger diameter than the rod.

Those last constructions seem to me to have besides the farther advantage, that any dilation of their parts from change of temperature, could only lengthen the tubes or rods, but would not alter their shape, on which alone their properties depend; which circumstance might perhaps make them even more exact than that proposed by Huygens, the strings, or appending part of which, would certainly vary in length; which, though, from the before-recited theorem, it appears to be a matter of little consequence, yet Huygens seems to hint that it would be of some, by recommending the use of fine chains instead of strings (that their length might vary the less), in these words: "*In locum fili catenulam tenuem exauro, aliove metallo, adhibere licebit, quo melius invariata servetur longitudo.*"

There has also occurred to me a species of oscillatory pendulum; which, as it appears to have the same property as those last mentioned, (of not being affected by the expansion of its materials,) I shall here describe: It consists of a cylinder of hard metal, A (*Fig. 4*) turned very true and smooth, placed so as to roll back and forwards alternately, in the cycloidal cavity B, cut in a block of hard metal also, and well polished: the communication of motion between the rolling cylinder and the clock work to be effected by the rod C, suspended by its upper extremity, and connected with each extremity of the axis of the cylinder by the joints D D: The diameter of the generating circle of the cycloid being half the length of a pendulum rod, which vibrates in the required time, and the cycloidal part well levelled and firmly sustained. It seems to me that the cylinder A, when put in motion, will oscillate equal times on its cycloidal support; and, (as the expansion of the latter will not alter its shape, on which alone its properties depend,) An oscillatory or rolling pendulum.

pend,) that the result will be the same in every different temperature; B may also be formed of two cycloidal plates, having circular grooves hollowed on their edges, and A have two projecting circles turned on it to run in those grooves; the method of connecting A with the clock-work may also be varied many other ways.

Concerning friction, &c.

Before I conclude, I beg leave to observe that the method, before mentioned, of preventing friction in the motion of the sphere in the parabolical trough, might be applied very beneficially in the construction of rail roads, by forming the upper part of the rail of a semi-cylindrical shape, and making in the edges of the wheels, (which were to run on them,) circular grooves, whose diameters should each exceed that of the rail; in the usual way of forming rail-roads, the lateral friction to the wheels is very considerable, whether they have flanches to them, and the rails are made plain, as in the Swansea roads; or the rails are formed with rising ledges, and the wheels plain, as in the Croydon road.

The circular pendulum a good regulator of first movers.

I shall also request permission to mention, in addition to your note on Prony's condenser of forces, that a regulator might be formed on the principles of the circular pendulum, as *Fig. 3*, which would temper the motion of wind-mills, or other engines, and react on them in return, (as well as the fly wheel), in a much more simple and effectual manner than the very complicated apparatus described by Prony for the same purpose, and which seems to me moreover peculiarly liable to be broken by any sudden increase of velocity in the mill, unless formed of most cumbersome strength.

I request the favour of having the following typographical errors noted in the paper on my tallow lamp. November, 1804, page 147, line the eighth, for the word *freely*, read *freely*; and line the 25th, same page, between the words *means* and *horizontal*, insert the letter P. In the plate also, the pan should be deeper and larger than there represented, and the tallow holder more central.

I am, Sir,

Your very humble servant,

J. WHITLEY BOSWELL.

Reference

Reference to the Figures.

Fig. 1. An exact copy of the figure of *Huygens's* circular pendulum, from the *Horologium*. B G A, the edge of the paraboloidal lamina: F E, the parabola produced by its means: F, the ball.

Fig. 2. The first construction proposed in place of *Fig. 1*. A C, the parabolical glass tube: A, the mercury: C, the air-vent: B B, the enclosing circular vessel represented in section.

Fig. 3. The third construction proposed instead of *Fig. 1*. A C, the metallic rod bent into a parabolical figure: B, the spherical weight: V, the roller on which it moves along A C: the parts in this figure are represented double, to shew the best method of adopting this plan as a regulator for wind-mills and other engines.

Fig. 4. The proposed construction for an oscillatory pendulum, which, it is imagined, will not be affected by change of temperature. A, the oscillating cylinder: B, the cycloidal support.

Fig. 5. A section of the trough mentioned in the second proposed construction, with the sphere in it.

Annotations. W. N.

Pyrometers, p. 74.] The common pyrometers of the shops are indeed liable to the objections of our author; but ingenious men have long ago obviated them in their experiments. Deluc, in the *Philosophical Transactions* for 1777, gives a method of ascertaining the relative expansions of two different metals by heat. He suspended one of the bars to an arm proceeding horizontally from an upright deal plank, and he supported the other bar by resting its lower end upon a small cock or stage proceeding from the lower end of the former. A microscope was attached to the plank in such a manner that, while it was constantly supported by an horizontal arm, it could be moved so as to keep the last mentioned or standing bar in the focus of radiation for distinct vision. Heat was applied to the bars, and the microscope was directed to a point on the standing bar. When by repeated shifting the microscope a point was found which was neither raised nor depressed by the changes of temperature, the respective lengths of the bars were inversely as their expansive powers. There was no mechanism, and the plank would remain at the same temperature during the experiments,

The best pyrometers do not give fallacious results.
Deluc's method.

riments, or might be kept so by means similar to those pointed out by our ingenious author, or his block of stone itself might have been used.

Ramsden's
pyrometer.

That great artist Ramsden, whose mechanical skill and clearness of intellect were so variously displayed in his works, and still more in his conversation to those who remember and regret that he has recorded so little of the results of his labours: this artist first availed himself of microscopes for ascertaining the terminal points of the subject under examination by his pyrometer; and the interval between one microscope and the other was rendered permanent by fixing them to arms proceeding at right angles from a bar of iron, kept at 32° by surrounding it with melting ice. Phil. Trans. Vol. LXXV.

Methods of
examining grid-
iron pendulums.

Magellan's in-
strument.

Lastly, another error, p. 74.] I believe the small spring and simple bar part, if any beneath the gridiron, are always taken into the account. These pendulums have not unfrequently been subjected to actual examination. An old friend of mine, Mr. J. H. de Magellan had an apparatus of tin, (Anno 1784) in which he inclosed the gridiron when to be examined. It was hung by its spring to an arm fixed in an upright plank of deal wood, against which plank were attached thermometers to shew its steadiness of temperature; and from the center of oscillation proceeded a (temporary) rod, the end of which presented a dot as the object to be viewed through a microscope duly attached to the plank. Without dwelling minutely on this apparatus, I need only observe that neither the arm of suspension, nor the support of the microscope, nor the rod from the center of oscillation had any parts but such as were parallel to the horizon, while the pendulum itself was perpendicular to it, and consequently that no error could arise but from expansion in the plank, which remained at the common temperature; and that the tin apparatus which covered and enclosed the pendulum had only three openings, one at the bottom, one at top, and one through which the rod from the center of oscillation passed, without touching the sides. In the experiment, steam from boiling water was admitted below, and when the heat was so raised, and the supply kept up, that uncondensed steam escaped above the pendulum was judged to be at 212° , and if the center of oscillation continued before the microscope without either rising or falling, the compensation was considered as complete.

Mr.

Mr. Troughton's apparatus, alluded to in his paper is different from those, and when he shall favour me with the account of it, and the experiments he is making, the world will see that it answers its purpose with such facility and precision as do honour to his great skill in these subjects.

Troughton's
pyrometer

Circular Pendulum of Huygens.] The difficulties attending the construction of this pendulum seem to be principally in the string or chain. Perhaps fine metallic wire might deserve the preference. It would not probably be difficult to bring the curve A B sufficiently near the true figure to answer its general purpose, through the changes of force in the first mover, and of resistance in the air. Practical men have not thought the cycloidal cheeks applied by Huygens, and since him by others, to the common pendulum, of any utility; but have rather directed their attention to small vibrations, or an equalizing of the first mover by periodically detaching the train. Huygens himself also proposed this expedient. It is generally admitted to be an advantage that the regulating instrument, whether pendulum or balance, should perform the greatest part of its motion unconnected with the train. The circular pendulum is constantly so connected.

Remarks on
Huygens's cir-
cular pendulum.

As the ingenious author has not detailed his reasons for thinking that pendulums of this kind will not be affected by change of temperature, I would simply remark that it does not appear to me why an expansion of the string should not cause the pendulum of Huygens to revolve more slowly, or that an expansion of the curves in the other figures would not diminish their curvature and produce the same retardation.

Whether it be
less liable to
change from
temperature.

The tube may be enclosed in a circular vessel.] As the resistance of the air will vary no less than a tenth or a fifteenth part according to the station of the barometer, we might expect time pieces to be considerably affected by its irregularity. In astronomical clocks, with heavy pendulums and short vibrations, this quantity is extremely minute, as is proved by their very correct performance. But there are facts attending the performance of our best portable chronometers, which shew that it is not in them an inconsiderable object. Several years ago I entertained a notion that the resistance of the air might be rendered equable, or in fact done away, by boxing up the balance, as is here proposed by our author. But from his own excellent paper on the blast ventilator inserted in the

On the method
of enclosing the
pendulum, &c.

fourth volume of the quarto series of this Journal, as well as from those of Professor Venturi, inserted in the same work, I learned how greatly the lateral reaction of the air, which is put in motion by bodies passing through it, must affect their movements. From this cause the value of the contrivance will undoubtedly be diminished, but I think not done away. I believe the famous pocket time-piece made about the year 1784, by Emery of Charing-Cross, for the President Baron at Paris, had its balance boxed up; but as I speak only from recollection, it is quite as probable that the whole movement was enclosed in a cap or case shut up by a secret catch.

III.

*Analytical Experiments and Observations on Benzoin. By Mr. WILLIAM BRANDE'. Communicated by the Author.**

Benjamin or benzoës little examined.

THE substance which forms the subject of the following experiments, and which is generally termed Gum-Benjamin or Benzoës, may be enumerated, among those objects, which till lately, have but little engaged the attention of chemists.

Mr. Hatchett's experiments.

We are indebted to Mr. Hatchett for almost all that is as yet known, as to the chemical properties of the resins, gum-resins and balsams; and as the substance in question, had not been examined by that gentleman, I was induced to make the following attempt.

Benzoin is obtained from the *styrax benzoe*.

Benzoin is obtained by incisions made in the tree called *Styrax Benzoe* †, from whence the balsam flows, one tree scarcely ever yielding more than three, or at the most four pounds.

Two kinds: viz. *benzoë amygdaloides*, and

Benzoin is generally divided into two kinds; the one is formed of whitish yellow tears, resembling almonds, united

* The following experiments were made merely with a view of ascertaining the effects of different menstrua on benzoin; but as the action of some of the acids, &c. would have been much less interesting, without a knowledge of the component parts of the balsam, I was induced to make some analytical experiments, which, though I fear they are by no means so accurate as they ought to be, will, I hope, still be found of some use.

† Dryander has given a description and drawing of this tree, *vide* Phil. Trans. 1787. p. 307.

together

together by a brown substance of the same nature, and is distinguished by the name of *benzoe amygdaloides*.

The other is a brown substance, somewhat resembling common ben-
mon resin, but possessing in other respects, the same qualities as
the former; it is called common benzoin.

The difference between these two species is said to arise from Cause of this
the latter having been exposed, for a length of time, to the difference.
sun, which converts the white benzoin into the brown.

The benzoin of the shops is usually in very large brittle Benzoin of the
masses, and such as is whitest, and free from extraneous sub- shops.
stances is the most esteemed. It grows in Sumatra, and is
brought from the East Indies only.

When chewed, it impresses a slight sweetness on the palate; Taste.
it has however but little taste. Its smell is fragrant and very Smell.
agreeable, becoming much more perceptible when gently
heated.

When thrown on hot coals it first fuses, then takes fire, emit- Effects of heat
ting at the same time a strong penetrating odour. Its specific on benzoin.
gravity, according to Brisson, is 1.092. Sp. gr.

Gren enumerates it among the resins *, as do most other
authors of his time; it is however more properly denominated
a balsam, since it is a combination of resin and benzoic acid.

We shall first examine the effects of different menstrua on resin and benzoic
benzoin, and then proceed to its analysis, together with the acid.
methods of obtaining benzoic acid.

§ I.

Effects of different Menstrua on Benzoin.

1. Cold water has but very little effect on benzoin; boiling Effects of water
water however extracts a part of its acid. on benzoin.

2. Alcohol dissolves the whole of benzoin when digested Of alcohol.
with it in a very gentle heat, the impurities remaining behind.
The solution is of a deep yellow colour, when perfectly satu-
rated, inclining to reddish brown, and possessing in some degree
the fragrant smell of the balsam, which may be obtained pure,
either by gentle evaporation of the solution or by the addition
of water, when a white powder precipitates, formerly known
by the name of *mâgistry of benzoin*.

The actions of the acids on the solution of benzoin in alcohol, Action of the
are very deserving of attention. acids on this
solution.

* Handbuch, § 1174. 2nd edition. 1794.

Of muriatic acid. A. Muriatic acid being added, a white curd is formed, and when three parts of acid and one of the solution are heated together, a very small part of the balsam is redissolved; which separates as the mixture cools, while the remainder is converted into a black brittle substance when cold, part of the benzoic acid having been dissipated by the heat and part dissolved, some of which separates in the form of a white scum, as the mixture cools, together with the benzoin.

Of sulphuric acid. B. Two or three drops of sulphuric acid added to the alcoholic solution, occasion a white precipitate, which disappears on the addition of a small quantity more of acid, the mixture assuming the appearance of port wine.

If however equal parts of the solution and of sulphuric acid be mixed together, a dark pink precipitate is formed, the fluid becoming likewise of the same colour, but the addition of water changes the whole to a lilac colour.

On evaporating a mixture of equal parts of solution of benzoin and sulphuric acid, the liquid retains its beautiful red colour till towards the end of the process, when it becomes gradually darker, and if the evaporation be carried on to dryness, a black coaly substance remains, a decomposition of the balsam having taken place.

Of nitric acid. C. Nitric acid added to the solution of benzoin in alcohol forms a dark red fluid, and when equal quantities are mixed, a violent effervescence takes place, attended with the emission of much nitrous gas. It must be observed that in this instance, no precipitate is formed, which proves that the balsam is soluble in nitric acid.

Of acetic acid. D. The effects of acetic acid do not exceed those of water.

Of acetic acid. E. Acetic acid forms a precipitate when added to this solution of benzoin. The effects produced by acetic acid on benzoin will be noticed hereafter.

Of the alkalies. F. The alkalies form no precipitate in this solution of benzoin, although the mixture in some cases becomes somewhat turbid.

Action of ether on benzoin. 3. Ether dissolves benzoin with great facility, the balsam being separated, when this solution is agitated with water.

Effects of the acids on this solution. The effects of the acids on this ethereal solution, so nearly coincide with those produced on the solution in alcohol, that there will be no need of a detailed account of them.

4. Nitric

OBSERVATIONS ON BENZOIN.

4. Nitric acid produces a violent effervescence when poured upon powdered benzoïn, the balsam being at the same time converted into an orange coloured mass. Effects of nitric acid on benzoïn.

Six ounces of nitrous acid, of the specific gravity of 1.36 being poured on one hundred grains of very pure benzoïn reduced into powder, the mixture being gradually heated in a sand bath till it boils, a solution of a light yellow colour is formed. The solution thus obtained, deposits a small quantity of benzoic acid, as it cools: this quantity gradually increases, till after some days, the whole of the benzoic acid appears to have been separated. Solution.
Separation of benzoic acid from,

This fact was first observed by Gottling; he does not however seem to have observed that the whole of the balsam is soluble in nitric acid. first observed by Gottling.

A. When the above solution, recently made, is poured into water, the benzoïn is precipitated, apparently unaltered. Effect of water on the nitric solution, of sulphuric acid,

B. Sulphuric acid causes no alteration in this solution.

C. Muriatic acid forms a white precipitate which is redissolved on the application of heat, forming a bright yellow liquid, which is a solution of benzoïn in nitro-muriatic acid, and in which no precipitate is formed either by water or the alkalies. Its colour is converted into dark brown, when excess of potash is added. acid, of muriatic acid.
Sol. of benzoïn in nitro-muriatic acid.
Effects of water and the alkalies on the nitro-mur. solution.

5. When sulphuric acid is poured on pulverised benzoïn, an effervescence takes place; part of the benzoïn is dissolved; forming a deep red liquid, and at the same time a coaly substance remains on the surface. Sulphureous acid gas is disengaged during the solution. Effects of sulphuric acid on benzoïn.

A. The alkalies form no precipitate in this solution, till after some hours standing, when a dark coloured precipitate is formed. Effects of the alkalies on this solution, of water.

E. Water produces a lilac precipitate in the recent solution. Effects of muriatic and acetic acids on benzoïn,

6. Neither muriatic nor acetic acids have any effect on benzoïn. of acetic acid.

7. Acetic acid dissolves benzoïn, even in the cold. The solution formed by the assistance of heat becomes very turbid on cooling, owing to the separation of the benzoic acid.

A. The alkalies form a white precipitate in this solution. The precipitate formed in the alcoholic solution, (page 11) is re-dissolved after some hours standing. Effects of the alkalies on this solution.

8. Acetic ether dissolves benzoïn, without the assistance of heat. Effects of acetic ether on benzoïn.

9. When

Effects of soda
and potash on
benzoin.

9. When a boiling lixivium of pure potash is poured on benzoin, a solution is immediately formed; the same effect is produced when pure soda is made use of. These solutions are of a dark brown colour, and become turbid after some days exposure to the air.

Effects of the
acids on these
solutions.

Precipitates are formed in them by the acids, which are re-dissolved on the application of heat, when nitric or acetic acids are added in excess. Ammonia likewise dissolves a small quantity of benzoin, from which solution it is precipitated by the acids.

Effects of am-
monia on ben-
zoin:

Effects of water
on the alkaline
solutions.

No alteration is produced when the alkaline solutions are largely diluted with water.*

§ II.

ANALYTICAL EXPERIMENTS ON BENZOIN.

Distillation of Benzoin.

Distillation of
benzoin.

One hundred grains of very pure benzoin were put into a glass retort, to which a tubulated receiver, and pneumatic apparatus were adapted; a very gentle heat was first applied, which was afterwards gradually increased, till the bottom of the retort became red-hot. The products thus obtained were as follows.

Component parts of ben- zoin.		Gr.
Benzoic acid	- - - - -	9.
Acidulated water	- - - - -	5.5
Butyraceous and empyreumatic oil	- - - - -	60.
Brittle coal	- - - - -	22.
And a mixture of carbonated hydrogen and car- bonic acid gas, which may be computed at	- - - - -	3.5

100.0

The quantity of benzoic acid above mentioned is that which may be separated by sublimation only, for on treating the oil, (which amounts to 60 grains) with water, five grains more of acid may be obtained, so that 100 grains of benzoin contain

Oil - - - 55

Acid - - - 14

* For an account of Mr. Hatchett's experiments on the resins, gum resins, and balsams, see Thompson's *System of Chemistry*, 2d edition, Vol. IV. page 305. for the resins and balsams to page 328. And from page 341 to 347 for the gum resins.

The greatest quantity of benzoic acid is obtained by Chaptal's method, which consists in distilling all the products over together, and separating the acid by means of boiling water. Chaptal's method of obtaining benzoic acid.

The acid, however, thus obtained, is by no means so pure as that procured by Scheele's process*, which is certainly far preferable in many respects. Scheele's process.

Another way of obtaining the benzoic acid, is that recommended by Götting. It consists in digesting the balsam, in a gentle heat with carbonate of potash and precipitation by sulphuric acid. Götting's process for obtaining benzoic acid. Gren made use of carbonate of soda: but it must be observed that in both these processes, the acid is by no means so pure as that obtained by Scheele's method, because a far larger proportion of the benzoïn itself is soluble in the fixed alkalies, than in lime water, (the latter only dissolving a very small portion, to which its brown colour is owing) and Gren's process.

* Scheele's process is as follows: "Upon four parts of unslacked lime pour 12 parts of water, and after the ebullition is over add 96 parts more of water; then put 12 parts of finely powdered benzoïn into a tin pan; pour upon it first about six parts of the above milk of lime, mix them well together, and thus successively add the rest of the mixture of lime and water. If it be poured in all at once, the benzoïn instead of mixing with it, will coagulate and run together into a mass. This mixture ought to be boiled over a gentle fire for half an hour, with constant agitation; then take it from the fire, let it stand quiet for an hour, in order that it may settle; pour off the supernatant limpid liquor into a glass vessel. Upon the remainder in the pans pour 96 parts of pure water, boil them together for half an hour, then take it from the fire and let it settle; add the supernatant liquor to the former; pour upon the residuum some more water, boil it as aforesaid, and repeat the same process once more. At last put all the residuums upon a filter, and pour hot water several times upon it. During this process, the calcareous earth combines with the acid of benzoïn, and separates it from the resinous particles of this substance. A small quantity of the resin is dissolved by the lime water, whence it acquires a yellow colour. All these clear leys and decoctions are to be mixed together and boiled down to 24 parts, which are then to be strained into another glass vessel. After they are grown cold, muriatic acid is to be added, with constant stirring, till there be no further precipitation, or till the mass tastes a little sourish. The benzoic acid which was before held in solution by the lime, precipitates in the form of a fine powder." Vide Thomson's Chemistry, Vol. II. page 123.

which



which is precipitated by the diluted sulphuric acid. The following table shews the quantity of benzoic acid, obtained by the different processes, from one pound of benzoin.

	Os.	Dr.	Scr.	Gr.
Scheele's process	-	-	-	1 6 2 19
Chaptal's	-	-	-	2 0 0 0
By boiling benzoin with water	-	-	1	0 0 10
By Gren's and Gottling's processes	-	1	5	1 10*

The acid of benzoin, first described by Blaise de Vigenere.

Benzoic acid is described by Blaise de Vigenere, as long ago as the year 1608, in his treatise on fire and salt; he called it flowers of benzoin, because it was obtained by sublimation. We are indebted to Tromsdorf and Lichtenstein for many facts relating to this acid.

Properties of the Acid of Benzoin.

Taste of the acid.
Smell.

This acid when obtained according to Scheele's process, is a light yellowish powder. Its taste is hot and rather bitter. It possesses a somewhat fragrant smell, arising from a small portion of the aromatic oil, which still adheres to it, it has however been obtained without any smell by Giese.

Properties in general.

It reddens tincture of turnsole. It is volatilized by a gentle heat, being converted into a white smoke, which excites a very disagreeable sensation in the throat. When melted, it becomes as fluid as water, and assumes a radiated surface on cooling. Its specific gravity is 0.667.

Decomposition.

When distilled in close vessels, a part of it is decomposed, which is converted into oil and carbureted hydrogen gas.—

Effects of oxygen gas, &c. on this acid.

Tromsdorf † found that it was not altered by oxygen gas, nor by the simple combustibles or incombustibles.

Of sulphuric acid.

It is soluble in sulphuric acid, which it converts into a brown liquid, and from which it is separated by the addition of water.

Of nitric and sulphurous and nitrous acids.

The same effects are produced by nitric acid, and by sulphurous and nitrous acids. It is not acted upon by muriatic, oxymuriatic, or phosphoric acids. It is easily soluble in alcohol,

Effects of muriatic acid, &c.

from which it is precipitated on the addition of water. Its

* Of pure acid, obtained by treating the precipitate with boiling water.

† See Tromsdorf's experiments on the benzoic acid, in his *Journal der Pharmacie*.

affinities appear to be as follows: *—White oxide of arsenic, Its affinities. potash, soda, ammonia, barytes, lime, magnesia, alumina.

Properties of the Oil of Benzoin.

The oil obtained by the distillation of benzoin possesses a strong empyreumatic odour; but when rectified by a second distillation, its smell is exceedingly fragrant and pleasant. Its *Smell and taste*, taste is acrimonious and very disagreeable.

When distilled with water, it imparts some of its taste to imparted to that fluid. It is perfectly soluble in alcohol, the solution be- water. coming turbid when water is added. Solution in alcohol.

Sulphuric acid decomposes it in part, when the mixture is heated. Nitric acid acts on it as on the essential oils in general, converting it into a resinous substance. It is not affected by muriatic acid, but is partially soluble in acetic acid. It sinks in water; and forms saponaceous compounds with the alkalies.

Such are the general properties of the acid, and oil of benzoin, much more might undoubtedly be said concerning them; but as the foregoing experiments, were not made with a view of investigating their properties, it would be unnecessary to give a long account of them in this place. It must be observed that the purest benzoin has been made use of throughout; and lastly, that no traces of alkali, were found in the coal which remains after distillation.

Arlington-Street.

IV.

On the Devitrification of Glass and the Phenomena which happen during its Crystallization. By DARTIGUES.

(Concluded from Page 64.)

WHEN clear glasses contain a certain quantity of a neutral salt which the fire has not had time or force to dissipate, it often happens, during slow cooling, that the appearances called grease, threads, bubbles, and specks, are spontaneously and suddenly formed.

* This table of affinities was formed according to the experiments of Tromsdorff. Vide Thomson's Systems of Chemistry, Vol. II. page 126. 2nd edition.

These

These accidents, their different causes and remedies, are treated at length in the first part of my work, where I speak of the fusing of glass; but though the explanation of these facts entirely belongs to the theory of devitrification, it will be sufficient in this place to mention the phenomena which arise from the presence of the different earths.

Crytallisation
of lime in glass,

Colourless glasses contain more or less of lime, for the reasons which I have mentioned in speaking of the different compositions of glass. This lime, when in excess, gives crystals very well, as Løysel remarks: they are easily known, and so abundant, that they absolutely impair the transparency: they are prisms, which seem to float in the midst of the paste of glass, and tend to unite in stars differently figured: these prisms are all nearly of the same thickness, and two or three millimetres in length (about one-tenth of an inch).

in large masses.

When this crytallization takes place spontaneously upon large masses at the bottom of the furnace, the colour of the glass becomes darker, and inclines to black, by the mixture of a certain quantity of the ashes. The striated stars here spoken of, become more numerous, the more remote they are from the side in contact with the fire. Crytallizations at first insulated, are soon succeeded by a mass entirely crytallized, in which the vitreous character is no longer distinguishable.

Modified crytall-
izations in glass.

These are the most common characters of crytallization; but others are frequently seen, which are certainly owing to accident, and well deserve notice on account of their varieties*. I have some pieces of glass in which crystals are discernable, so fine, that they can scarcely be seen with a magnifier. They are prisms divergent from the same center, and forming stars, which frequently do not exceed one millimetre (or half a tenth of an inch) in diameter: their union resembles a slight mist in the paste of the glass.

Some pieces have the aspect of a saline crust applied to a foreign body, against which the glass was in contact. In some of these this crust being composed of striated protuberances, appears to advance more and more into the glass itself.

Lastly, there is another variety perhaps more curious than all the rest, in which centers, or crytallizations resembling

* Mr. Sage possesses a piece of glass crytallized in six-sided prismatic prisms, totally devitrified.

peas, and almost similar to grain, are seen in the midst of the paste of glass. They are small spheres flattened at each end with an indentation in the middle of each depression. The sides have facets, like the grain of Indian nasturtium, and these facets are always six in number.

I intend, when I shall have procured a sufficient number of this singular species of crystal, to analyze it, in order to determine which of the earths it is that effects so extraordinary a form.

I have thus given a short account of the principal facts which characterize the precipitation and crystallization of glass. It is evident that they are of the same nature as those produced by the cementation pointed out by Reaumur; and that this cementation is always a more or less absolute devitrification of the glass.

When the glass is devitrified, it has no longer a vitreous Recapitulation. but a granulated fracture; it has no transparency, and perfectly resembles a stone; it becomes a less bad conductor of heat and of electricity; lastly, it is no longer susceptible of fusion at the same degree of heat; and in order to restore it more easily to Revitrification. the vitreous state, it must first be pulverized, to bring into contact those substances which, during the crystallization, had become separated from each other, and could no longer serve as fluxes to each other.

I hope the novelty of many of the facts I have here indicated, and the consequences I have drawn from them, may be sufficiently interesting to philosophers to have some claim to their attention. I have no doubt but that other general inductions will present themselves to them which may have escaped me, or which could not be introduced in a short memoir. The resemblance of my specimens of devitrified glass with those of certain lavas; the possibility that other lavas may have undergone a more complete devitrification by a much slower cooling beneath the volcanic currents which are known to have flowed, or to have remained fluid for whole years beneath congealed crusts;—every fact leads me to think, that these phenomena may afford a solution of many geological events, concerning which no general opinion has yet been adopted, because there was no reason to believe that stones may have formerly possessed the vitreous state.

General conclusion and application to geology.

V.

Letter from a Correspondent, relating to the apparent Reflection of Cold and the Invention of the Telescope, as noticed by Sir H. C. ENGLEFIELD in last Month's Journal.

To Mr. NICHOLSON.

SIR,

IN justice to Professor Piclet, I trust you will have the goodness to insert in your next number the words of a note in his *Essai de Physique*, p. 81, where he describes his experiment on the apparent reflection of cold :

Apparent reflection of cold.

“ *Les Académiciens del Cimento avoient essayé de concentrer le froid au foyer d'un miroir concave; mais ils avoient eux-mêmes, que leur expérience avoit été faite d'un manière trop inexacte pour qu'on pût en rien conclure de certain.*”

Perhaps Professor Piclet speaks too lightly of the experiment of the Academicians; but so far was that experiment from being forgotten, that the account of it has been very lately reprinted in the first volume of the Journals of the Royal Institution, p. 224; a work to which your correspondent, who has revived the subject (page 1 of your last Number), has himself contributed some valuable papers.

Kepler did not invent or know of the telescope before Galileo.

With respect to the invention of the telescope, give me leave to observe, that *perspicillum* means simply the sight of an astronomical instrument, having a small perforation. If the comet in question was surrounded by a small round and well-defined nebulosity, such a sight might easily produce the effect attributed to it. A telescopic sight is usually called *perspicillum vitreum*. Galileo says, in a work published in 1610, that he was informed of the invention of telescopes about nine months before. Kepler wrote, in 1611, a valuable work on the theory of dioptrics, in which the construction of the astronomical telescope is described. Had he been the original inventor of the Galilean telescope, he would not omitted this opportunity of substantiating his claim to so important a discovery.

I am, Sir,

Your very obedient servant,

ALETES.

Facts

VI.

Facts tending to decide the Question, at what Point of Temperature Water possesses the greatest Density. In a Letter from Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

ACCORDING to the doctrine in most of our books treating of the elements of chemical science, it is considered as a decided fact, that water is of the greatest density at or near 40° of temperature of Fahrenheit's scale; and that, above or below that temperature, it expands alike by heat or cold for a certain number of degrees. I have taught a different doctrine for some time past; which is, that water is densest at 32° , or the freezing point, and that it expands alike above and below that point for at least 25° (provided it does not freeze), and that the quantity of expansion is as the square of the temperature, reckoned from 32° , up or down; that is, if the expansion from 32 to 42° be denoted by 1, that from 32 to 52° will be denoted by 4, and that from 32 to 62° by 9, &c. or nearly so; the deviation from strict accuracy being, as I conceive, occasioned by the mercurial thermometer not being an exact measure of temperature: the expansion at 22° is likewise 1, at 12° it is 4, and at 2° it is 9, &c.; the same below as above the freezing point. In consequence of this, a gentleman of professional eminence has undertaken to examine the subject anew; and has been led, by an ingenious train of experiments totally different from the following, to decide in favour of the common notion, that water is densest about 40° . These experiments of his will soon be published. I am still, however, decidedly convinced, that my opinion above stated is true, and that principally from the facts I am about to state, which those who adopt the common opinion must see the absolute necessity of explaining upon their principle, or otherwise, of controverting the facts themselves. As this season of the year is most convenient for experiments of this nature, I am the more solicitous to have the facts announced, leaving all explanation for the present. They are of a very simple nature, and may be investigated with little trouble and expence.

General notion that water is densest at 40° , &c.

The author's doctrine that 32° is the point of greatest density.

Experiments brought to support the former conclusion.

The author retains his opinion, and invites other experiments.

A number

Instructions for experiments.
Procure large bulb thermometers, of earthen ware, glass, metal, &c.

A number of water thermometers are to be procured, the containing vessels of which are of different materials, as earthen ware, glass, and various metals. Each of these should contain one or two ounces (from 4 to 800 grains), more or less, of water. Common brown inkstands, which go by the name of Nottingham ware, answer very well for one species, but they require to be well painted without, as they are not otherwise water-tight. I have a few of Queen's ware, made purposely in Staffordshire, which constitute another species of earthen ware; some of them are glazed in and out; others unglazed, but these being painted without are made water-tight, and expand the same by heat as glazed ones: Of glass, common thermometer tubes, with larger bulbs than ordinary, are sufficient. I have the metallic vessels made in the shape of cylindrical tin canisters, conical towards the top, and at the summit a small cylindrical tube, such as to take a thermometer tube. The glazed earthen ware and the metallic, require mostly to be painted before they are quite tight.

Fill them with boiled water.

The vessels being thus prepared, they are to be filled with water previously boiled to expel the air; a thermometer tube with cement is then suddenly plunged into the vessel and cemented fast; the water may then be driven out of the tube by heat, or more may be put into it by a small wire; it is then fit for use, and a scale of equal parts may be applied to the tube; or it may be divided, and marks made with a file or paint.

Some of the results of my experiments with instruments of this kind, are as follow:

		Water lowest.	Water the same height.
Experimental results noted with three earthen, one glass, two iron, one copper, one brass, one pewter, and one leaden thermometer.	1. Brown earthen ware, No. 1, at 36°		at 32° and 40°
	2. Brown earthen ware, No. 2, - 38	- 38	- 32 and 44
	3. Queen's ware, - - - 40	- 40	- 32 and 48
	4. Flint-glass, - - - 41½	- 41½	- 32 and 51
	5. Iron, thin plate, - - 42½	- 42½	- 32 and 53
	6. Tinned iron, - - - 42½	- 42½	32 and 53
	7. Copper, - - - 45½	- 45½	32 and 59
	8. Brass, - - - 46	- 46	32 and 60
	9. Pewter, - - - 46	- 46	32 and 60
	10. Lead, - - - 49½	- 49½	32 and 67

Another

MR. HATCHETT'S EXPERIMENTS ON LAC.

Another phenomenon in these instruments is observable; it is not new, but it deserves a marked attention in the present enquiry: If the apparent expansion of water for the first 10° of temperature, reckoned from the lowest point in any of the above instruments, be denoted by 1; then if the instrument, taken at any temperature, be suddenly plunged in water of 10° higher temperature, the water instantly *sinks* a considerable way, occasioned no doubt by the vessel being extended by the heat before the water it contains has time to expand. The quantity of depression in the different instruments was found as under:

Brown earthen ware sinks by being dipped in			
water of 10° higher temperature, -			
Queen's ware,	-	-	.2
Flint-glass,	-	-	.3
Iron, -	-	-	.25
Copper,	-	-	.66
Brass,	-	-	.9
Pewter,	-	-	1.1
Lead,	-	-	1.0
			1.5

Table of this depression in the different thermometers.

I submit these facts to the consideration of those who feel interested in the enquiry; and desire that they may be particularly investigated by those who maintain that water is densest at 40°.

I remain your's,

J. DALTON.

Manchester, Jan. 10, 1804.

VII.

Analytical Experiments and Observations on Lac. By CHARLES HATCHETT, Esq. F. R. S. From the *Philosophical Transactions* for 1804.

(Concluded from p. 54.)

Properties of the colouring Extract of Lac.

1. **W**HEN dry, it is of a deep red colour, approaching to purplish crimson. Colouring extract of lac.
2. Being put on a red-hot iron, it emits much smoke, with a smell somewhat resembling burned animal matter, and leaves a very bulky and porous coal.

3. Water,

Colours of the
Solutions.

3. Water, when digested with it in a boiling heat, partially dissolves it; but the residuum was found to be absolutely insoluble in water.

4. Alcohol acts but slowly on it; and, in a digesting heat, dissolves less than water. The colour of the solution is also not so beautiful; and a considerable part of the residuum left by alcohol was, when digested with water, found to be soluble, although this was not the case, when the residuum left by water was treated with alcohol.

5. It is insoluble in sulphuric ether, excepting a very small portion of resin, which appeared to be accidentally mixed with it.

6. Sulphuric acid readily dissolves it, and forms a deep brownish-red solution, which, being diluted with water, and saturated with potash, soda, or ammonia, becomes changed to a deep reddish-purple.

7. Muriatic acid dissolves only a part: the solution is of the colour of port-wine, and, by the alkalis, is changed to a deep reddish-purple.

8. Nitric acid speedily dissolves it: the solution is yellow, and rather turbid; but the red colour is not restored by the alkalis, for these only deepen the yellow colour. This nitric solution did not afford any trace of oxalic acid.

9. Acetic acid dissolves it with great ease, and forms a deep brownish-red solution.

10. Acetous acid does not dissolve it quite so readily, but the solution is of a brighter red. Both of the above, when saturated with alkalis, are changed to a deep reddish-purple.

11. The lixivium of potash, soda, and ammonia, act powerfully on this substance, and almost immediately form perfect solutions, of a beautiful deep purple colour.

12. Pure alumina, put into the aqueous solution, does not immediately produce any effect; but, upon the addition of a few drops of muriatic acid, the colouring matter speedily combines with the alumina, and a beautiful lake is formed.

13. Muriate of tin produces a fine crimson precipitate, when added to the aqueous solution.

14. A similar coloured precipitate is also formed, by the addition of solution of isinglass.

These properties of the colouring substance of lac, especially its partial solubility in water and in alcohol, and its insolubility in ether, together with the precipitates formed by alumina and muriate of tin, indicate that this substance is vegetable extract, perhaps slightly animalized by the cocculus.

The effects which it produced on gelatin, also demonstrate the presence of tannin; but this very probably was afforded by the small portions of vegetable bodies, from which the stick lac can seldom be completely separated.

Properties of the Resin of Lac.

This substance is of a brownish-yellow colour; and, when Resin of lac. put on a red-hot iron, it emits much smoke, with a peculiar sweet odour, and leaves a spongy coal.

It is completely soluble in alcohol, ether, acetic acid, nitric acid, and the lixivium of potash and soda.

Water precipitates it from alcohol, ether, acetic acid, and partially from nitric acid; and it possesses the other general characters of a true resin.

Properties of the Gluten of Lac.

It has been already observed, that when small pieces of shell Gluten of lac. lac have been repeatedly digested in cold alcohol, they become white, bulky, and elastic. By drying, these pieces become brownish and brittle; the elasticity is also destroyed by boiling water, exactly as when the gluten of wheat is thus treated.

If the pieces of shell lac, after the digestion in alcohol, be digested with diluted muriatic acid, or with acetic acid, the greater part of the gluten is dissolved, and may be precipitated, in a white flaky state, by alkalis; but, if these last be added to excess, and heat be applied, then the glutinous substance is redissolved, and may be precipitated by acids.

If the pieces of shell lac, after digestion in alcohol, be treated with alkaline lixivium, then the whole is dissolved, and forms a turbid solution. But, when acids are employed, the chief part of the gluten is alone acted upon, and a considerable residuum is left, consisting of the wax, some

of the resin, and a portion of gluten, which has been protected from the action of the acid by the two former substances.

The above properties indicate a great resemblance between this substance and the gluten of wheat; I therefore have called it gluten, but, at a future time, I intend to subject it to a more accurate examination.

Properties of the Wax of Lac.

Wax of lac.

If shell lac be long, and repeatedly digested in boiling nitric acid, the whole is dissolved, excepting the wax, which floats on the surface of the liquor, like oil, and, when cold, may be collected; or it may be more easily obtained in a pure state, by digesting the residuum left by alcohol in boiling nitric acid.

The wax thus obtained, when pure, is pale yellowish white, and (unlike bees wax) is devoid of tenacity, and is extremely brittle.

It melts at a much lower temperature than that of boiling water, burns with a bright flame, and emits an odour somewhat resembling that of spermaceti.

Water does not act upon it, neither does cold alcohol; but this last, when boiled, partially dissolves it, and, upon cooling, deposits the greater part; a small portion, however, remains in solution, and may be precipitated by water.

Sulphuric ether, when heated, also dissolves it; but, upon cooling, nearly the whole is deposited.

Lixivium of potash, when boiled with the wax, forms a milky solution; but the chief part of the wax floats on the surface, in the state of white flocculi, and appears to be converted into a soap of difficult solubility; it is no longer inflammable, and, with water, forms a turbid solution, from which, as well as from the solution in potash, the wax may be precipitated by acids.

Ammonia, when heated, also dissolves a small portion of the wax, and forms a solution very similar to the former.

Nitric and muriatic acids do not seem to act upon the wax; the effects of sulphuric acid have not been examined.

When the properties of this substance are compared with those of bees-wax, a difference will be perceived; and, on the contrary,

contrary, the most striking analogy is evident, between the wax of lac and the myrtle wax which is obtained from the *Myrica cerifera*. It greatly resembles myrtle wax.

An account of the latter substance has been published by Dr. Bostock, of Liverpool, in Nicholson's Journal, with comparative Experiments and Observations on bees-wax, spermaceti, adipocire, and the crystalline matter of biliary calculi.*

The properties of the myrtle wax, as described in Dr. Bostock's valuable paper, so perfectly coincide with those which I have observed in the wax of lac, that I cannot but consider them as almost the same substance; indeed I think they may be regarded as absolutely identical, if some allowance be made for the slight modifications which have been produced by the different mode of their formation.

From the preceding experiments and analyses we find, that the varieties of lac consist of the four substances which have been described, namely, extractive colouring matter, resin, gluten, and a peculiar kind of wax. Refin is the predominant substance; but this, as well as the other ingredients, is liable, in a certain degree, to variation in respect to quantity. Component parts of lac.

According to the analyses which have been described, one hundred parts of each variety of lac yielded as follows:

Stick Lac.

Refin	-	-	-	68.
Colouring extract	-	-	-	10.
Wax	-	-	-	6.
Gluten	-	-	-	5.50
Extraneous substances	-	-	-	6.50
				<hr/>
				9.60.

Seed Lac.

Refin	-	-	-	88.50
Colouring extract	-	-	-	2.50
Wax	-	-	-	4.50
Gluten	-	-	-	2.
				<hr/>
				97.50.

* Nicholson's Journal for March, 1803, p. 129.

Shell Lac.

Refin	-	-	-	90.90
Colouring extract	-	-	-	0.50
Wax	-	*	-	4.
Gluten	-	-	-	2.80
				<hr/>
				98.20.

The proportions of the substances which compose the varieties of lac, must however be subject to very considerable variations; and we ought therefore only to consider these analyses in a general point of view. Hence we should state, that lac consists principally of resin, mixed with certain proportions of a peculiar kind of wax, of gluten, and of colouring extract.

The relative quantity of the two latter ingredients, very considerably effect the characters of the lacs; for instance, we may observe, that the glutinous substance, when present in shell lac in a more than usual proportion, probably produces the defect observed in some kinds of sealing wax, which, when heated and burned, become blackened by particles of coal; for the gluten affords much of this substance, and does not melt, like the resin and wax. From what has been stated, therefore, lac may be denominated a *cero-resin*, mixed with gluten and colouring extract.

§ III.

GENERAL REMARKS.

Lac has the characters of vegetables.

From the whole of the experiments which have been related, it appears that although lac is indisputably the production of insects, yet it possesses few of the characters of animal substances: and that the greater part of its aggregate properties, as well as of its component ingredients, are such as more immediately appertain to vegetable bodies.

It is very useful; Lac, or gum lac, as it is popularly but improperly called, is certainly a very useful substance; and the natives of India furnish full proofs of this, by the many purposes to which they apply it.

for rings, beads; According to Mr. Kerr, it is made by them into rings, beads, and other female ornaments.

When

When formed into sealing wax, it is employed as a japan, ^{sealing wax,} and is likewise manufactured into different coloured var-^{varnishes;} nishes.

The colouring part is formed into lakes for painters: a sort lakes, dying; of Spanish wool for the ladies is also prepared with it: and, as a dying material, it is in very general use.

The resinous part is even employed to form grindstones, by ^{grindstones,} and melting it, and mixing with it about three parts of sand. For ^{polishers;} making polishing grindstones, the sand is sifted through fine muslin; but those which are employed by the lapidaries, are formed with powder of corundum, called by them Corune.*

But, in addition to all the above uses to which it is applied ^{the Hindû ink;} in India, as well as to those which cause it to be in request in Europe, Mr. Wilkin's Hindû ink occupies a conspicuous place, not merely on account of its use as an ink, but because it teaches us to prepare an aqueous solution of lac, which probably will be found of very extensive utility.

This solution of lac in water may be advantageously ^{em-water varnish:} employed as a sort of varnish, which is equal in durability, and other qualities, to those prepared with alcohol; whilst, by the saving of this liquid, it is infinitely cheaper.

I do not mean however to assert that it will answer equally well in all cases, but only that it may be employed in many. It will be found likewise of great use as a vehicle for colours; for, when dry, it is not easily affected by damp, or even by water.

With a solution of this kind, I have mixed various colours, ^{The latter very useful.} such as vermilion, fine lake, indigo, Prussian blue, sap green, ^{Instances.} and gamboge; and it is remarkable, that although the two last are of a gummy nature, and the others had been previously mixed with gum, (being cakes of the patent water-colours,) yet, when dried upon paper, they could not be removed with a moistened sponge, until the surface of the paper itself was rubbed off.

In many arts and manufactures, therefore, the solutions of lac may be found of much utility; for, like mucilage, they may be diluted with water, and yet, when dry, are little if at all affected by it.†

We

* Phil. Transf. 1781, p. 380.

† The alkaline solutions of lac are evidently of a saponaceous nature, and, like other soaps, may be decomposed by acids. The entire

**Refins acted on
by acids and al-
kalis.**

We find, from the experiments on lac, that this substance is soluble in the alkalis, and in some of the acids. But this fact (considering that resin is the principal ingredient of lac) is in opposition to the generally received opinion of chemists, namely, that acids and alkalis do not act upon resinous bodies. Some experiments, however, which I have made on various resins, gum-resins, and balsams, fully establish, that these substances are powerfully acted upon by the alkalis, and by some of the acids, so as to be completely dissolved, and rendered soluble in water.

Field of inquiry. It will be a very wide and curious field of inquiry, to discover what changes are thus produced in these bodies, especially by nitric acid. Each substance must form the subject of a separate investigation; and there cannot be a doubt but that much will be learned respecting their nature and properties, which hitherto have been so little examined by chemists.

**Utility of resin-
ous solutions.**

The alkaline solutions of resin may be found useful in some of the arts; for many colours, especially those which are metallic, when dissolved in acids, may be precipitated, combined with resin, by adding the former to the alkaline solutions of the latter. I have made some experiments of this kind with success; and perhaps these processes might prove useful to dyers and manufacturers of colours. It is probable also, that medicine may derive advantages from some of this extensive series of alkaline and acid solutions of the resinous substances.

entire substance of lac is not however completely dissolved, as appears from the turbidness of the liquors. Three of the four ingredients, namely, the resin, the gluten, and the colouring extra appear to be in perfect solution; whilst the wax is only partially combined with the alkali, and forms that imperfectly soluble saponaceous compound which has been formerly mentioned, and which remains suspended, and disturbs the transparency of the solution.

From various circumstances, it does not seem improbable, that the long sought-for, but hitherto undiscovered vehicle employed by the celebrated painters of the Venetian School, may have been some kind of resinous solution, prepared by means of borax, or by the alkalis.

VIII.

A simple and accurate Method of Surveying on Shore, with such Instruments only as are in every one's Possession. By Captain J. MORTLOCK. From the Author.

To Mr. NICHOLSON.

28, Surry Place, Kent Road,
7th January, 1805.

SIR,

ALMOST all our treatises on nautical surveying begin with the explanation and description of what are called the necessary instruments, which are described so numerous, and the price so considerable, that very few can procure them: Thus discouraged, they abandon every idea of making plans of such ports as they touch at, for want of what they conceive to be the necessary instruments. Introductory letter.

To obviate this difficulty, and to render nautical surveying more general, I have, in the annexed paper, attempted to shew the mariner how to survey any port or place he may touch at, with great accuracy, little trouble, and without any expence for instruments. Should you find this simple method deserving of a place in your valuable Journal, I shall feel myself honoured by your inserting it.

I am, Sir,

Your most obedient humble servant,

J. MORTLOCK.

FIRST make an eye-sketch of the place to be surveyed, as the annexed figure, numbering all the points, bays, rocks, shoals, &c. Choose two stations, as A and B (*Fig. 1, Plate V*), whence all the rocks, points, &c. may be seen from, and so situated from each other, that the bearings of the points, &c. as taken from A and B, shall intersect at angles at least greater than 10 degrees, but the nearer 90 degrees the better. Nautical surveying without instruments.

Having chosen the stations, proceed to one of them as A, and place the paper intended to receive the plan horizontally before you, extended by pins, or otherwise, on a board securely fixed, to prevent it shifting its position while the bearings are drawing.

Stick

Nautical surveying without instruments.

Stick a pin through the paper firm into the board, at the part meant to represent the station A, and lay a ruler with a perfect straight edge on the paper, touching the pin at A and pointing towards the station B, and draw the line A B: in like manner draw lines from A towards all the points, rocks, bays, &c. numbering the lines as the points, rocks, bays, &c. are numbered in the eye-sketch: Proceed next to the station B, and place the board horizontally before you, so that the line A B shall point back towards A, and secure the board with the same precaution as at A, to prevent its shifting: then, in the line A B, stick a pin firm through the paper into the board, in that part meant to represent the station B; from which point draw lines pointing towards the different points, rocks, &c. as was done from A, numbering them in like manner. Now, where the lines drawn from B intersect those of the same number drawn from A, will be the place of the points, rocks, &c. to which the lines were directed to from the stations. Sketch in the shore* between the points, &c. and the plan is completed.

The meridian-line may be found by compass, or more correctly, by drawing the line of the sun's bearing from one of the stations, and taking his altitude at the same time. Then with the latitude, altitude, and declination, compute the azimuth, and lay it off to the left or right of the line of the sun's bearing, according as the sun was to the right or left of the meridian, and it will give the true north and south, or meridian-line.

If the distance between any two points on the shore be measured, it will give you a scale for the plan; but it may often be found more convenient to measure off a base, as A C, from one of the stations, in a direction nearly perpendicular to the line A B; and let it be in length equal to some part of a geographical mile, as 380 feet = $\frac{1}{16}$, or 760 = $\frac{1}{8}$, or 1520 = $\frac{1}{4}$, or 3040 = $\frac{1}{2}$, or any part of a mile; then will the line A C be a scale to the plan.

I have supposed any common board and ruler to illustrate the simplicity of this method of surveying: but to such as are provided with a drawing frame, it will be found convenient to extend the paper on; and if a ruler has sights perpendicular to its edge, it will be found commodious, and require less trouble. I hope the ease and expedition with which the whole

is performed, will induce sea-faring people to amuse themselves by taking plans of the places they touch at: for it is by the improvement of geography that the dangers of navigation are diminished, and, consequently, the lives and property embarked in our shipping are less exposed to danger.

J. MORTLOCK.

IX.

Notice of an Omission in Accum's Chemistry, of the direct Production of Nitric Acid. By W. F. S.

To Mr. NICHOLSON.

SIR,

Lincoln's Inn, January 7, 1805.

HAVING in your Philosophical Journal spoken very handsomely, and not undeservingly so, of Mr. Accum's Treatise on Chemistry, it would not perhaps be improper in one of your ensuing Numbers, either to elucidate or correct a small difficulty, or rather error, in that book. In describing the mode of producing nitric acid, in the second volume of Mr. A.'s work, p. 286, he puts as a principal ingredient *nitrate of potash*: Now, on referring to the mode of obtaining *nitrate of potash* in page 287, and following page, it is described as being produced by neutralizing the carbonate of potash with *nitric acid*: Now, Sir, it is pretty evident, that the means of preparing either nitric acid or nitrate of potash is not given, or rather it is stated by *implication*, as *incapable of being produced by art*.

Nitre and nitric acid in Accum's chemistry.

Very respectfully your's,

W. F. C.

P. S. As this error can only be rectified by a subsequent edition, it would, with submission, be extremely beneficial to the holders of the present edition, that you should introduce the emendation in your Journal.

X.

On Galvanism. By Mr. CHARLES SYLVESTER,

To Mr. NICHOLSON.

SIR,

Difficulties respecting the galvanic decomposition of water; as to the distances of the wires.

YOU did me the honour of inserting in your valuable Journal, some experiments tending to illustrate the theory of galvanism. I do not know who originally proposed the idea of the combination of electricity with hydrogen (the truth of which my experiments were intended to establish), though I am now bound to acknowledge the ingenuity and importance of the thought. Such an idea would perhaps never have been suggested, had it not been for the very paradoxical appearances attending the decomposition of water by galvanism. The appearance of the hydrogen at so great a distance from the oxygen, both of which must have been produced from the same particle of water, was very satisfactorily accounted for by this conjecture. The continental philosophers, and Dr, Gibbes of Bath, did not so well account for the phenomena by their hypothesis, though it appeared so formidable, as to threaten the theory of modern chemistry with dissolution.

Hypothesis of Mr. Wilkinson censured.

Another hypothesis of the decomposition of water, was given by Mr. Wilkinson, in his Elements of Galvanism, and in your 36th Number. If we even allow Mr. W. the advantage of all his very gratuitous data in accounting for the decomposition of water, the contradiction with which they abound, will totally render his hypothesis invalid.

Remarks in detail on Mr. W.'s hypothesis.

Mr. W. begins by supposing a particle of water analogous to the Leyden phial, which is the same thing as to suppose that water, a conductor of electricity, is composed of particles in themselves non-conductors. Mr. W. is of opinion, that the decomposition takes place in the middle of the liquid between the points of the wires. The way in which he supposes the separation to be effected, is something like the idea the ancients had of the solutions of metals in acids, viz. that the metal was split into very minute particles (capable of being suspended in the liquid) by the wedge-like particles of the acid. After the decomposition is effected, he tells us that the capacity of the hydrogen is diminished for electricity, while

while the capacity of oxygen is increased for the same substance; two facts for which I should be glad to know Mr. W.'s authority. He now supposes the oxygen to be attracted by the positive wire, where it saturates itself with the electricity necessary to constitute its gaseous form. The hydrogen, on the other hand, is attracted to the negative wire, to which it must give its excess before its elastic form can be effected.

After all the labour Mr. W. has bestowed to bring the hydro-Objection. gen to one wire, and the oxygen to the other, he does not appear, in my opinion, to have succeeded; for the excess of electricity in the hydrogen would be given to the deficient oxygen, and the gasses would be given out in the middle of the liquid, and not at the ends of the wires.

Mr. W. mentions the curious fact of water not being de-
 composed in a very small tube, and also, that no decompo-
 sition takes place when the wires in the liquid are as much
 as eight inches distant from each other. I have long been in
 possession of these facts: I have varied these experiments by
 using, instead of pure water, different solutions of salts, with
 a view to increase the conducting power of the liquid me-
 dium: I found a solution of carbonate of potash to answer
 very well: I placed the wires in the ends of a tube of more
 than a yard in length: The decomposition went on very ra-
 pidly: I soon observed the positive wire coated with beau-
 tiful carbonate of copper, at the same time I observed a ga-
 seous substance disengaged: I found it, by the test of lime-
 water, to be carbonic acid gas. I observed the disengage-
 ment of this gas, after I had rendered the potash very caustic
 by lime.

I afterwards used a tube of $\frac{1}{16}$ th of an inch in diameter, and five feet long, into which I introduced a saturated solution of common salt. After the wires were introduced, and the communication made, I observed bubbles of hydrogen upon the negative wire, in about a minute after *.

* I remain, Sir,

Your's, &c.

CHARLES SYLVESTER.

Sheffield, Jan. 20, 1805.

* This corroborates the fact of the hydrogen passing invisibly through the liquid.

XI.

Reply to the Animadversions and Experiments of C. L. on the Subject of the Horizontal Moon. By Mr. EZRA WALKER.

To Mr. NICHOLSON.

DEAR SIR,

WHAT can have induced your correspondent C. L. to attempt to confute my paper relating to the horizontal moon, with so much invidiousness, is best known to himself. For my own part, I believe that he is personally unknown to me, and that I am equally so to him.

Whether the telescope be properly urged against Mr. W.'s theory.

The first step that this fastidious writer takes to disprove the truth of my position is a false one. He says, that "if Mr. Walker's position were true, the magnifying power of the same telescope would vary with its aperture." *

C. L. has been very unfortunate in mentioning the telescope, for the properties of that instrument will confute every argument which he has used against my theory, and show that his ill conducted experiments, like an *ignis fatuus*, tended only to mislead him.

The want of a standard would prevent its being seen whether a less light gives a greater appearance to objects.

I shall drop this subject for the present, to examine his next assertion in the same paragraph. The apparent magnitude of the paper before the eye may become larger, when the candle is shaded with the hand, for any thing that C. L. knows to the contrary, as the increase is too small to be perceived by our senses; and even if it were ten times larger than it is, it could not be known, because every other object before the eye would increase in apparent magnitude at the same time, and in the same ratio; and consequently leave no standard to compare the paper with. To elucidate this in a familiar way, permit me, Sir, to ask how would C. L. determine the number of miles between Troston in Suffolk, and Soho-Square in London, without some standard measure, with which to compare that distance.

Strictures on C. L.'s experiments.

Then follow his "correct experiments of the same description as mine."—My experiments were made to imitate the eye, upon a large scale. Let us see how C. L. has imitated nature in his experiments.

* Philos. Journal, Vol. IX. p. 235.

In his first experiment, "from the flame to the lens the distance was $49\frac{1}{2}$ inches, and from the lens to the image it was 58 inches." These numbers being reduced to the scale of the human eye, it will appear that the distance between the crystalline lens and the retina, is nearly twice as great as between the crystalline lens and the object; consequently, if we take the distance between the crystalline and the outward surface of the cornea into the calculation, the object must nearly touch the eye. This, I think, may be called a *short-sighted experiment*.

Exp. 1. The object was too near the lens.

The conclusion of C. L.'s paper contains a number of particulars, in which he *supposes* I have erred, and then modestly "submits it to yourself and readers, whether I have acted consistently with the rules of philosophical investigation," &c.

But as it will appear hereafter, that none of those errors had any existence, except in C. L.'s own mind, his conclusion can serve no other purpose than to shew the temper and disposition of the writer.

In my paper which was honoured with a place in the 9th vol. of this Journal, page 164, I only gave an abstract of a series of experiments, on which I founded my general conclusion; but it now appears necessary to give a more particular account of them, to shew that they are entitled to more credit than C. L. has thought fit to give them.

A more particular account of the author's former experiments.

After having prepared my apparatus as described in my paper above-mentioned, with a long mould candle of six to the pound, placed in an inclined position, I began with measuring the focal image of the whole lens.

The length of this luminous picture upon the screen, was determined by a pair of compasses, and a diagonal scale of inches. This measure was entered on page 1 of a book, prepared for that purpose.

The card No. 1. was then placed before the lens; the luminous picture measured in the same manner as before, was entered on page 2 of the same book.

The measure of the picture given by the aperture, No. 2. was entered on the 3d page. And the measure of that given by No. 3. was noted down on the 4th page.

As soon as these four experiments were finished, I began again with the whole lens, and entered a second measure on page 1. under the first, and continued till I had obtained two measures

measures on each page: and in this manner, I continued my experiments until I had obtained five or six measures to each aperture. This mode gave me an opportunity of seeing how much the flame of the candle altered in its length, during the time that I was making the experiments, which alteration was too small to be regarded. The measures on each page being added up, and divided by the number of them, gave me those numbers which were inserted in my paper.

But I did not stop here, for sets of experiments were repeated in the same manner, on several evenings, to ascertain the fact more clearly. But to come more immediately to the point; when the sun or moon was used instead of a candle, the same result was obtained, viz. the greatest aperture gave the greatest luminous picture.

Principles of optics brought in support of the author's theory.

The pencils of light that enter the eye are smaller, the smaller the aperture of the object lens;

The truth of this property in optics, however, does not wholly rest on my experiments, the same conclusion may be derived from other principles.

It is a well known property of the telescope, that "as the aperture is contracted, the slender pencils or cylinders of rays that emerge from the eye-glass into the eye, are also contracted in the same proportion." *

—whence it is inferred that the moon's picture will be equally changed in size.

The magnifying power, of the object glass of a telescope is not increased by increasing its aperture; but by increasing its aperture, you increase the magnitude of the pencil of light in its focus: for as the eye-glass remains the same, the increase in magnitude of the emerging pencil of rays, must depend upon an increase in magnitude of the pencil in the focus of the object glass. Consequently, when the moon is viewed, if the aperture of the object glass be increased, the picture of the moon within the telescope, will be increased in the same ratio; but without increasing the magnifying power of the instrument.

For the magnifying power is = the diameter of the aperture of the object glass divided by the diameter of the emerging pencil.

Demonstration.

Let A = the diameter of the aperture of the object glass,
 B = the diameter of the emerging pencil, C = the focal dis-

* Elementary Parts of Smith's Optics, page 93.

tance of the object glass, and D the focal distance of the eye-glass. Then $\frac{C}{D}$ = the magnifying power.

But $C : D :: A : B$. (Smith's Optics, p. 93.) And by division $\frac{C}{D} = \frac{A}{B}$. Therefore the enlargement of the picture of an object within the telescope, does not increase its magnifying power.

Hence these properties of the telescope prove to a demon Conclusion.
stration, that the conclusion which I have drawn from my experiments is a law of vision; and this law shows us, that no concavity of the sky; no terrestrial perspective, nor even the painting of the waves under Black-Friar's Bridge, can explain the phenomenon in question *. The cause lies wholly within the eye.

It is a most unpleasant task to enter into controversy, but I cannot see how I could avoid it in this instance. It appears to me, Sir, that you inserted C. L.'s paper for me to answer, and I have complied; hoping, however, that this is the last time of my having occasion to use my pen, against an anonymous writer. Common justice seems to demand the names of those who undertake the examination of such papers as are owned by their respective authors.

I am, Dear Sir,

Your most obedient servant,

EZ. WALKER.

Lynn, Jan. 18, 1805.

XII.

Description of a simple Instrument for making correct Drawings from Nature. By T. C. B.

To Mr. NICHOLSON.

SIR,

January 22, 1805.

THE description of two instruments for facilitating landscape drawing from nature given in the first volume, page 221, of your Journal, has suggested to me the construction of

* Philosophical Journal, vol. IX. page 237.

another,

**Instrument for
making correct
drawings from
nature.**

another, which seems to unite the accuracy of the first of those instruments with the simplicity and portability of the second. As I conceive it may be of some service to those who are in the habit of sketching landscape, I send you a description of one which I have had made, that you may, if you think proper, publish it in your Journal. Let A B, *Fig. II. Plate 5.* be a flat rule, suppose twelve inches long, having at its extremities two arms B D and A F turning upon a joint at B and A; and in each arm a circular joint at C and G: let the length of each arm B D and A F be ten inches: a handle to fix on E, and a thread passing through two holes equidistant from the handle, making any length, according to the angle under which the view may be best seen.

To use the instrument, take the end of the thread in the mouth, and hold the instrument upright before the eye, then move either or both of the arms till the points D and F are brought in a line between the eye and any point in the landscape you may wish to delineate; lay the instrument upon the drawing paper, and you will have the true situation of such part of the subject.—Proceed in like manner, taking care always to keep the base line in the same place, till you get as many points as you require, by which means any landscape or building may be drawn very expeditiously, and with a great degree of accuracy.

To make the instrument as portable as possible, there is a joint in A B, which the handle covers, and the pieces D G and C F are made of thin brass, to fold into the pieces A C and B G; so that the instrument, when folded up, need occupy no more room in the pocket than a small spectacle case.

It is perhaps unnecessary to add that this instrument may be used for the purpose of copying, and answers the purpose of a triangular or quadrangular compass.

I am, Sir,

Your most obedient and humble servant,

T. C. B.

If the rough inclosed sketch of the instrument is not sufficiently intelligible, Mr. Nicholson may see one at Mr. Frazer's, Optician, in Bond-Street.

XIII.

Observations on Basalt, and on the Transition from the vitreous to the stony Texture, which occurs in the gradual Refrigeration of melted Basalt; with some geological Remarks. In a Letter from GREGORY WATT, Esq. to the Right Hon. CHARLES GREVILLE, V. P. R. S. From the Philosophical Transactions for 1804, p. 279.

SIR,

Soho, April 1804.

THE important geological consequences that seem deducible from the changes of texture developed by Sir James Hall's very judicious experiments on the regulated cooling of melted basalt, induced me to attempt a repetition of them, some time after the publication of his interesting and ingenious paper. * I believe that formerly I had the honour of showing you some of the results of my imperfect and diminutive experiments, which only served to afford additional proof of the transition from the vitreous to the stony texture, which takes place in the gradual refrigeration of glass. Circumstances have prevented my resuming these investigations, till it lately occurred to me that something might be learned, by exposing to the action of heat, a much larger mass of basaltic matter than, as far as I am informed, had ever at one time been subjected to experiment.

One of the common feverberatory furnaces used in iron foundries for the fusion of pig-iron, was strongly heated by a fire maintained for several hours. About seven hundred weight of amorphous basalt, here called Rowley Rag, was broken into small pieces, and deposited gradually on the elevated part of the interior of the furnace, between the fire and the chimney, from whence, as it melted, it flowed into the deeper part, in which, in ordinary operations, the melted iron is collected. It was observed by the persons attending, that it did not require half the quantity of fuel to fuse the basalt, that would have been necessary to melt an equal weight of pig-iron. When the whole was melted, it formed a liquid glass, rather

Sir James Hall's experiments on the slow congelation of fused basalt.

The experiment repeated on 7 cwt. of rowley rag.

It was easily fused, and formed perfect glass when quickly cooled.

* Published in the Transactions of the Royal Society of Edinburgh, Vol. V. and in our Journal, Vol. V. quarto series.

Vol. X.—FEBRUARY, 1805.

I tenacious,

Slow congelation.

Appearance of the mass.

tenacious, from which a large ladle-full was taken, which, on being allowed to cool, retained the characters of perfect glass. The fire was maintained, though with gradual diminution, for more than six hours; after which time, the draught of the chimney was intercepted, the surface of the glass was covered with heated sand, and the furnace was filled with coals, which were consumed very slowly. It was eight days before the mass in the furnace was sufficiently cool to be extracted, and even then it retained considerable internal heat.

The form of the mass, being given by the bottom of the furnace, was considerably irregular, approaching to the shape of a wedge whose lower angles were rounded. It was nearly three feet and a half long, two feet and a half wide, about four inches thick at one end, and above eighteen inches at the other. From this diversity of thickness, and from the unequal action of the heat of the furnace, too great an irregularity had prevailed in the refrigeration of the glass, to permit its attainment of a homogeneous texture. These circumstances might probably have been counteracted by better devised precautions; but the inequality of the product is not to be regretted, since it has fortuitously disclosed some very singular peculiarities, in the arrangement of bodies passing from a vitreous to a stony state, which might have remained unobserved, if the desired homogeneity of the result had been obtained. I shall now endeavour to describe the various products of this operation; and I shall also submit to your consideration, some remarks which appear to me to arise naturally from the phenomena I have observed; premising that, except where my opinions are supported by the unequivocal demonstration of facts, I offer them with the utmost deference to the decision of more experienced and judicious mineralogists and geologists.

External characters, &c. of rowley lag.

It may be proper to give a concise description of rowley lag itself before I consider the products which it yields by igneous fusion. This species of basalt is fine-grained, of a confused crystallized texture; its fracture uneven in small pieces, conchoidal in large pieces. Its hardness superior to common glass but inferior to feldspar. Its tenacity considerable. Its action on the magnetic needle strong, but without signs of polarity. Its specific gravity, according to my trials, 2.868. Its general colour iron gray, approaching to black. It is opaque; and it reflects

reflect light from a number of brilliant points, some of which seems to be felspar, and the others hornblende.*

1st. This substance is easily fused into glass, whose texture is its glass. completely vitreous, with few air-bubbles. Its fracture undulated conchoidal. Its hardness superior to felspar, but inferior to quartz. It possesses scarcely any action on the magnetic needle. Its colour is black: it is nearly opaque, being translucent only in very thin fragments. Its specific gravity appears to be 2.749.

2d. The tendency towards arrangement in the particles of the fluid glass, is first developed by the formation of minute globules, which are generally nearly spherical, but sometimes elongated, and which are thickly disseminated through the mass. The colour of these globules is considerably lighter than that of the glass; they are commonly grayish-brown, sometimes inclining to chocolate brown, and, when they have been formed near the interior surface of the cavities in the glass, they project, and resemble a cluster of small seeds. Their diameter rarely exceeds a line, and seldom attains that size, as, in general, they are so near to one another, that their surfaces touch before they can acquire considerable magnitude. In the process of cooling, they adapt their form to their confined situation, fill up every interstice, and finally present a

First appearance of arrangement. Small globules appear in the mass.

* "The ragstone has been accurately analysed by Dr. Withering, who found that 1000 parts of it contained 475 parts of siliceous earth, 325 argillaceous earth, and 200 calx of iron; but this iron seems to me to be in a very small degree of calcination, from the dark blue colour of the stone, from the rusty colour it assumes on being exposed to a farther state of calcination by air and water, and from the magnetic property of the mountain, which, as Dr. Plot observed, turned the needle 6° from its proper direction. This magnetic property has since been observed in several basaltic mountains, particularly in the Giant's Causeway in Ireland, and very remarkably in a basaltic columnar mountain called Compals Hill, in the island Cannay, one of the Hebrides, described by George Dempster, Esq. in the Transactions of the Society of Antiquaries in Scotland, Vol. I." See Mineralogy of the South-west part of Staffordshire, by James Keir, Esq. F. R. S. published in Shaw's History of Staffordshire, Vol. I.

Analysis of rowley rag by Dr. Withering.

Mr. Kirwan states the specific gravity of rowley rag, which he calls *ferrillite*, at 2.748; and assigns its melting point at 98° of Wedgwood's pyrometer.

homogeneous body, wholly unlike glass, and equally unlike the parent basalt. When the union of the little globules has been imperfectly effected, the fracture of the mass indicates its structure, by numerous minute conchoidal fractures, which display the form of each globule. But, if the arrangement has extended a little farther, all these subdivisions are entirely lost; the mass becomes perfectly compact, has an even or a flat conchoidal fracture, is nearly of the same hardness as the glass, is commonly of a chocolate colour, graduating into a brownish-black, and the intensity of the colour increases in proportion to the degree to which the arrangement has extended. Its aspect is rather greasy; and it much resembles some varieties of jasper in the compactness of its texture, and in its opacity. Its magnetic action is extremely feeble. Its specific gravity appears to be 2.938.

It resembles jasper, and has little magnetic action.

More advanced appearance of the stony texture.

Darker colour.

Large spheroids of radiated texture.

3d. If the mass were now rapidly cooled, it is obvious that the result would be the substance I have just described; but, if the temperature adapted to the farther arrangement of its particles be continued, another change is immediately commenced, by the progress of which it acquires a more stony texture, much greater tenacity, and its colour deepens as these changes advance, till it becomes absolutely black. Sometimes this alteration is effected by a gradual transition, the limits of which cannot be assigned, but more generally by the formation of secondary spheroids, in the heart of the compact jaspideous substance. These spheroids differ essentially from those first described; the centres of their formation are more remote from each other, and their magnitude is proportionably greater, sometimes extending to a diameter of two inches, and seeming only to be limited by contact with the peripheries of other spheroids. They are radiated, with distinct fibres; sometimes the fibres resemble those of brown hæmatites, and sometimes they are fasciculated irregularly, so as to be very similar in appearance to the argillaceous iron ores rendered prismatic by torrefaction. They are generally well defined, and easily separable from the mass they are engaged in; and often the fibres divide at equal distances from the centre, so as to detach portions of the spheroid in concentric coats. The transverse fracture of the fibres is compact and fine grained; the colour black; and the hardness somewhat inferior to that of the basaltic glass. When

two of the spheroids come into contact by mutual enlargement, no intermixture of their fibres seem to take place; they appear equally impenetrable, and, as neither can penetrate, both are compressed, and their limits are defined by a plane, at which a separation readily takes place, and each of the sides is invested with a rusty colour. When several spheroids come in contact on the same level, they are formed by mutual pressure into pretty regular prisms, whose division is perfectly defined; and when, a spheroid is surrounded on all sides by others, it is compressed into an irregular polyhedron.

4th. The transition from this fibrous state to a different arrangement, seems to be very rapid; for the centre of most of the spheroids becomes compact, before they attain the diameter of half an inch. As the fibrous structure propagates itself by radiating into the unarranged mass, the compact nucleus which supplies its place gradually extends, till it finally attains the limits of the spheroids; and the same arrangement pervades the matter comprehended between them. The mass has now assumed a compact stony texture, and possesses great tenacity. Its hardness is somewhat inferior to that of the glass from which it was formed. Its action on the magnetic needle is very considerable. Its specific gravity is 2.938. Its colour is black, inclining to steel gray: it is absolutely opaque, and only reflects light from a few minute points. Though the divisions between the spheroids are rendered imperceptible to the eye, they are not obliterated, and their rusty surfaces are often disclosed by an attempt to fracture the mass.

5th. A continuation of the temperature favourable to arrangement, speedily induces another change. The texture of the mass becomes more granular, its colour rather more gray, and the brilliant points larger and more numerous: nor is it long before these brilliant molecules arrange themselves into regular forms; and, finally, the whole mass becomes pervaded by thin crystalline laminæ, which intersect it in every direction, and form projecting crystals in the cavities. The hardness of the basis seems to continue nearly the same; but the aggregate action, of the basis, and of the imbedded crystals, on the magnetic needle, is prodigiously increased. It appears to possess some polarity; and minute fragments are suspended by a magnet. Its specific gravity is somewhat increased, as it is now 2.949. The crystals contained in it, when

Contact with
spheroids pro-
duces polyhe-
drons.

Third arrange-
ment. Compact
stony texture,
great tenacity,
opaque, consider-
able magnetic
action.

Granular tex-
ture.

Crystalline lami-
næ.

Polarity.

Increased den-
sity.

when examined by a microscope, appear to be fasciculi of slender prisms, nearly rectangular, terminated by planes perpendicular to the axis; they are extremely brilliant; their colour is greenish-black; they are harder than glass, and fusible at the blow-pipe; they are suspended by the action of a magnet. They are arranged nearly side by side, but not accumulated in thickness, so that they present the appearance of broad thin laminæ; they cross one another at all angles, but always on nearly the same plane; and the lamina thus formed is often three or four lines long, and from a line to a line and a half broad, but extremely thin.*

It seems obvious, that an equalized temperature would have rendered the whole similar to the substance last described; and it may be fairly inferred, that by a continuance of heat, the minute crystals would have been augmented in their dimensions, by the accession of molecules still engaged in the basis, or by the union of several crystals, till they acquired sufficient magnitude for their nature to be absolutely determined by the usual modes of investigation. It is probable, however, if such precautions had been taken as might have secured this degree of perfection in the ulterior result, that the mass would only have exhibited an uniform aspect, and that the interesting initial phenomena would not have been discovered.†

There

* It may be observed, that the cavities which existed in the glass are not obliterated during the subsequent processes, though their interior surfaces undergo some change. The minute globules first formed often become prominent, and project into the cavities. These minute points are soon obliterated by the large curves of the fibrous spheroids, which give a mamellated form to the interiors of the cavities; and, when the crystals are generated in the mass, they shoot into some of the cavities, and line them with their brilliant laminæ.

† In this and the succeeding paragraphs, the word molecule is used in the sense assigned to it by Haüy and Dolomieu, and is understood to represent the peculiar solids, of definite composition and invariable form, the accumulation of which, forms the crystals of mineral substances. Such molecules, preserving their form and their essential characteristics, may be extracted from most crystals by mechanical division, and may be subdivided as far as our senses can recognise them. Though we cannot by mechanical means directly divide them into their elementary particles, we are enabled

to

There are some considerations which appear to offer a partial explanation of the formation of the globules, and of the radiated spheroids. It is well ascertained that heat is emitted by all bodies, in their change from a gaseous to a fluid state, and also in their change from a fluid to a solid state. It is reasonable to suppose, that heat may also be emitted in those changes of arrangement which affect the internal texture of a body, after it has attained an apparently solid state. That a succession of such changes does actually take place, appears to me demonstrated by the appearances I have described, and by the increase of specific gravity, which seems to keep pace with the internal changes of the substance. It would appear, that these changes are caused by a gradual diminution of temperature, which permits certain laws to induce peculiar arrangements among the particles of the glass. When several of these particles enter into this new bond of association, they must form a minute point, from which heat must issue in every direction. That heat will gradually propagate itself, till the temperature of the glass is equalized; and then the recurrence of the circumstances which induced the first particles to arrange, will cause other particles to arrange also, which the attraction of aggregation will dispose round the point first formed. A second emission of heat in every direction will take place; the temperature will again be equalized; and again another concentric coat of arranged particles will apply itself to the little globule. But, at the time when the central point of this globule was formed, the equality of temperature, in the mass of glass, would probably cause a number of similar points to be generated. The formation of each must proceed in a similar manner to what I have described, till their surfaces touch, and all the glass be converted into the same substance.

Explanation of the formation of the globules and spheroids; from the emission of heat during congelation.

These globules are therefore formed of concentric coats, but they are also radiated. Every one must have remarked to effect this by chemical solution, the only power to which their aggregation yields. It will be evident, from the observations that follow, that I am inclined to adopt the ingenious idea of Dolomieu, that many apparently homogeneous rocks are compounds of the minute molecules of several species of minerals; and that, where a suitable opportunity is given, these will develop themselves by the formation of their peculiar crystals.

General connection between the radiated structure and formation by concentric coats.

the connexion that almost uniformly exists, between the radiated structure and the formation by concentric coats. There are few radiated substances which are not divisible into concentric fragments; and as few concentric arrangements which are not radiated. Of the first, it may be sufficient to mention hæmatites; of the second, calcareous stalactites. The tendency to this union of structure, may perhaps be produced by the radiation of the emitted heat, or moisture, if the solution be aqueous; and the divisions of the coats will naturally take place at those pauses in the accumulation of particles, which the momentary emission of heat necessarily induced.

Formation of the
larger spheroids
explained.

If this be allowed to explain the formation of the first series of globules which consolidate into the jaspideous substance, it will also explain the formation of the larger and more distinctly radiated spheroids, which have been already stated to be very easily divisible into concentric fragments. They probably were also formed round a central point, by the accumulation of thin coats; and the tendency to radiation, which seems almost inseparable from this structure, was perhaps aided by the arrangement induced by the emission of heat from every part of the surface of the spheroids. This mode of formation has the advantage of explaining their impenetrability. Had they been generated by radii diverging from a centre, their compactness must have diminished as their diameter increased; but, in the structure which I have supposed, each coat is composed of particles solidly arranged in immediate contact with each other, leaving no spaces for penetration. The same progress is rigidly observed in the extension of the compact nucleus, which always occupies the centre of the radiated spheroids, and finally extends to their peripheries. It observes the concentric divisions of the radiated part with the greatest precision; and the line of their separation is always perfectly defined. But the state of aggregation into which the substance has now entered, is so perfect as to overcome the operation of the causes which formerly induced the fibrous structure, and the mass remains compact. The only change that the substance afterwards undergoes, consists in the gradual accumulation of the crystalline molecules, and their arrangement, by their individual polarity, into regular solids. This depends on very different laws from those

those which consolidated the fluid glass, and aggregated its particles into a compact uniform stone.*

The appearances that I have endeavoured to describe, seem deserving of consideration in several points of view. Few things can be more at variance with commonly received opinion, than the diversified succession of changes of structure which this glass exhibits in its passage to a crystallized state. The generation of the globules which unite to form the jaspideous substance, is what we might be prepared to expect, by observing the cooling of a common iron furnace slag. But it appears not very obvious to common apprehension, that the species of arrangement requisite to form this intermediary substance, could be compatible with any fluidity permitting farther motion of the molecules of the mass; yet, immediately after the completion of this arrangement, they receive a new disposition, and the radiated fibrous structure commences. Sometimes this pervades even the unaltered glass; but I presume this only to happen where the minute globules first formed were scattered so far asunder, that their centres became fibrous, before their peripheries came into contact. Other considerations. Remarkable circumstance, that these changes succeed after fluidity may be supposed to have ceased, &c. This view of the subject is justified by the analogous operation of

* The case is considerably different, where crystals possessing regular forms are generated in glass. The molecules of which they are formed, have doubtless been only suspended in the vitreous medium; and their union is determined by crystalline polarity, which appears to me perfectly distinct from the simple aggregation which changes a fluid into a solid, whether it be homogeneous or compound, which affects the internal arrangement of those bodies, but which never can separate their components into distinct masses, or form them into regular solids. Every molecule, at the moment of its formation, must necessarily be endowed with all the properties it afterwards possesses. The suspension of such molecules in a fluid medium, though it may conceal, cannot alter those properties; and the union of such molecules, to form a regular solid, in no respect alters their individual or aggregate qualities. Whether heat be evolved at the moment of this union, is a question not easily solved; as the crystallizations with which we are familiar are from chemical solutions, in which some of the molecules are generated by the separation of a combined substance, at the moment when others are united by crystalline polarity.

the formation of crystals, similar to those described, in the heart of the radiated spheroids, while their exteriors still retained the fibrous texture.

— Review of the facts under this point of view.

If it be considered as extraordinary, that a change should be effected, converting an apparently solid and homogeneous mass into an accumulation of radiated spheroids, and that these radii should lose their fibrous structure, and assume the texture, aspect, and tenacity, of a compact, hard, and homogeneous stone, it is certainly much more extraordinary, that this stone should permit farther arrangement to proceed, and should enable the crystalline molecules which it contains in a state of confused aggregation, to arrange themselves, and to form crystals which, although minute, are equal in the perfection of their forms, and in the brilliancy of their natural polish, to the most precious products of crystallization. It is also well deserving of observation, by how regular a march the magnetic influence of the substance keeps pace with the perfection of its arrangement, till it becomes so powerful, that fragments of the regenerated stone are suspended by the attraction of a magnet.

The act of crystallization is incompatible with solution or fluid combination.

It has been most justly remarked by Mr. Smithson, that solution, far from being necessary to crystallization, effectually prevents its commencement; for, while solution subsists, crystallization cannot take place. It may remain a question, whether previous solution be essential, as a preparatory means of obtaining, by subsequent evaporation, or cooling, the small parts of bodies disengaged, so that they may unite to form regular crystals. If by solution be only meant, that simple action of heat, or water, which merely counteracts the force of aggregation, and relieves the molecules from their bonds of union with each other, it certainly is a requisite; but if by solution be meant, that action of affinities by which not only the force of aggregation is overcome, but the combinations which constitute the molecules are destroyed, it obviously is not only unnecessary but prejudicial to crystallization; as a new set of molecules must be formed, by a new combination of the elementary particles, before the formation of regular bodies can commence.

Suspension is necessary; but this may be called mechanical.

The suspension of the molecules ready to crystallize, may be correctly said to be merely mechanical. Though the mechanical action of trituration can never be expected to resolve even the most easily divisible body in its molecules, because the

the fractures will be at least as frequently across the natural joints as in their direction, yet, even by this rude method, some perfect molecules may be disengaged; for we find, that water passing over large surfaces of siliceous sand, finds some molecules of flint in the state proper for aggregation, and even for crystallization. Mechanical suspension in a fluid medium, of such density that the crystalline polarity may be enabled to counteract the power of gravity, is with justice considered by Mr. Smithson the only requisite for the formation of crystals. * The circumstances I have detailed, appear to me an additional confirmation of this remark, and perhaps go still farther, by showing that even the fluidity (in the common sense of the word) of the suspending medium is not an indispensable condition. For it appears impossible to annex the idea of fluidity to the union of the minute globules which form the jaspideous substance, still less to that substance when formed, and still less to those spheroids whose obstinate impenetrability is so strongly defined. And if, by any power of imagination, these can be supposed to be fluid at the time they retain this conformation, how can it be supposed that the compact and tenacious stone into which they are changed could retain these characters in a fluid state? Yet the subsequent formation of crystals proves, that either all these contradictions must be, or that the particles of bodies apparently solid must be capable of some internal motion, enabling them to arrange themselves according to polarity, while they are solid and fixed, as far as they have reference to the ordinary characters of fluidity.

Even fluidity does not seem an indispensable condition.

Instances even more remarkable have very long been known and authenticated, though perhaps they have not been generally regarded with the attention then deserve. Glass vessels are well known to be convertible into Reaumur's porcelain, by the internal arrangement of their particles, without losing their external form, and consequently at a temperature very much below that requisite for their fusion. The change of glass into Reaumur's porcelain, does not arise from an evaporation of the alkali, as has been alledged, but from a regular arrange-

Instances of the internal motions and arrangements of particles of bodies at temperatures below fusion.

Reaumur's porcelain fully described:

* See a chemical Analysis of some Calamines, by James Smithson, Esq. Philosophical Transactions for 1803, page 27. See also Delomieu, *Journal des Mines*, No. 22, page 53.

ment of the molecules of the glass. It commences by the formation of fibres perpendicular to the surface of the glass, and penetrating into it. At nearly the same time, small radiated globules are formed in the interior of the glass, and the union of these with the fibres, by their mutual increase, forms the whole into a new substance; and, if the requisite temperature be longer maintained, the fibres disappear, and the whole becomes fine-grained, and almost compact. This substance, from the improved state of its aggregation, is much stronger and more tenacious than before, and is not fusible at a heat sufficient to fuse the glass it was formed from; but, if that aggregation be once destroyed, the glass resulting from its fusion is equally fusible with the original glass; and a repetition of the process will again form Reaumur's porcelain, which may be again fused, and so on repeatedly, for the quantity of alkali evaporated during the operation is extremely small. The hardness and brittleness of metals rapidly cooled, contrasted with the softness and tenacity resulting from their gradual refrigeration, are all analagous instances; and all the processes in which annealing is employed, and more remarkably the tempering of steel, the proofs of the internal motions and arrangements of the particles of matter, at temperatures very much below the heat is requisite for their fluidity.

hardening;
tempering; an-
nealing;

Crystallization
of a mass of the
same nature
throughout;

Whatever doubts may arise respecting the formation of the crystals, there seems no reason to suppose that their gradual increase would cease, till all the molecules belonging to that species were exhausted, if the temperature favourable to their generation was continued. If the mass was entirely composed of one species of molecules, it would be resolved into an aggregation of crystals of the same substance; and probably by a still farther continuation of the process of arrangement, into one crystal, which, though it might not possess a regular external form, would be perfect in its internal structure.

of particles dif-
fering from each
other.

But, if the mass contains two distinct species of molecules, different results must take place, which will be modified by the proportional quantities of the components. As it has been demonstrated by Berthollet, that the attraction of masses of matter are relatively as their quantities, it follows, that unless a very potent counteracting cause be exerted, the most abundant ingredient in the mixture will be the first to crystallize.

Generally, the
most abundant
ingredient will
crystallize first.

But this crystallization will not comprehend the whole of its molecules; for, after a certain quantity of them are arranged, the proportions of the remaining fluid are altered; that ingredient which was before the least, may now be equal, or even greatest, and it will exercise its attraction. As the first crystallization, by subtracting a large portion of the fluid particles, must have obliged the molecules of the less abundant substance to approach each other very closely, they may be able to collect themselves entirely in their first attempt to crystallize, or they may form alternate crystallizations with the remaining unarranged molecules of the more abundant substance. How-Diversities ever various the species of molecules may be, they will be regulated by analogous laws, and only serve to diversify the generated substances.

It by no means follows, that the crystals afterwards found to be most infusible would be first generated. Their formation does not altogether depend on their greater or less fusibility, but on the relative strength of the attraction which unites them to the matter they are immersed in, and of the polarity which invites them to crystallize. In all crystallization from compound fluids, the order in which the several bodies crystallize must be determined by their relative quantities and attractions. It is perfectly obvious, that no molecules can form a crystal in a heat sufficient for its fusion; but it by no means ensues, that it will be formed as soon as the molecules are cooled to the point where the crystalline polarity overcomes the dissolving power of heat; for they may remain suspended in a fluid formed by more fusible bodies, provided this fluid be sufficiently abundant to keep them from contact with each other, for the crystalline polarity appears to exert itself only at extremely small distances. In a mass composed of substances in a state of fluidity, with refractory molecules suspended among them, it is pretty clear, from the preceding paragraph, that the most abundant ingredient will be the first to crystallize. But the removal of a portion of the suspending fluid must bring the refractory molecules nearer together, and perhaps so near that the crystalline polarity may overcome the attraction of the fluid for them; they will therefore crystallize next, and will be followed by the remaining ingredients, in the order their attractions dictate.

The most infusible crystals would not necessarily be formed first.

When refractory substances crystallize after more fusible matters, they may have the impression of these last.

No crystal more fusible than its enveloping mass, can be formed by igneous operation.

Aqueous solution and suspension do not differ from those in the dry way. They are effected by the fusion of the solid water, or ice, &c.

As the crystals last formed must necessarily be impressed, at the parts in contact, by the peculiar forms of those which have been first generated, it also follows, if the preceding reasoning be just, that the infusible crystals may be found impressed by the more fusible substance, which crystallized first; and the remaining ingredients of the mixture, which were subsequently arranged, may be moulded on the refractory crystals; and thus, in the same specimen, may exist a refractory substance generated by fire, impressed by more fusible bodies, and impressing them in its turn. From the same consideration it is obvious, that no crystal can be formed at a temperature above the degree of its fusibility; and that, as a necessary consequence, no crystal which is more fusible than the basis in which it is imbedded, can be formed by igneous operation.

The same laws must regulate the arrangement of aqueous solutions, and of molecules suspended in aqueous solutions. All these are dependant on heat; for we are unacquainted with any fluidity; and consequently with any solution, which heat does not produce. Ice and soda have no more action on each other than soda and quartz: raise the temperature of the ice, and it unites with the soda; raise the temperature of the soda, and it unites with the quartz. Both solutions are effected by heat, of the degrees of which we know neither the beginning nor the end, and are therefore utterly unable to estimate what aliquot part of its scale is adequate to the production of these effects. Probably a very minute one,

(To be continued.)

XIV.

*Account of the Method of bleaching Cotton, as practised at Salzb-
burgh; and the Art of giving a permanent Red to Cotton and
Linen. By M. C. SCHOERBING.**

Salzburg method
of bleaching cot-
ton thread, &c.

COTTON thread is always washed before it goes to the weaver. The method of washing here to be described gives it a much whiter colour than ordinary; and it is equally appli-

cable to cottons in the piece. This method is called, for what Salzburgh method of bleach-
 reason I know not, the Salzburgh method, though it is chiefly ing cotton
 practised at Reginsburgh, where four families have long kept thread, &c.
 it a secret, which has not till the present occasion been divulged.
 This washing renders the cotton more flexible; the goods made
 from it are of a closer and more even texture; the cotton does
 not require any subsequent bleaching, and when it comes at-
 terwards to be dyed, it takes a finer, more permanent and more
 uniform colour. The operation is performed with soap, water
 and strong leys, in the following manner:

The articles to be washed, which usually consist of thread,
 stockings, night caps, and handkerchiefs, are evenly disposed
 in a large vessel or tub. The bottom of this vessel is first cov-
 ered with a coarse linen cloth, upon which are laid first the
 handkerchiefs, afterwards the stockings and night-caps, and
 lastly the thread. The whole is then covered with a second
 coarse cloth of a close texture, and very clean, which defends
 the mass from the contact of the air. The vessel itself resembles
 those used for lixiviation in the soap work of Konsholm. A
 boiler having its diameter at the top $1\frac{1}{2}$ Swedish ell (forty
 inches) and at the bottom $1\frac{1}{2}$ ell (34 inches) and its depth one
 ell (27 inches) is filled with water to which a sufficient quan-
 tity of caustic leys of potash is added to make the fluid produce
 a greasy feel between the fingers, and afterwards 2lbs. of
 sliced soap of Rigenburg. This liquid is made to boil and
 poured upon the cotton; a short time after which it is drawn
 off, to be again heated, during which interval a hot solution
 of soap is continually poured upon the cotton. These oper-
 ations are repeated for four or six hours, or until the cotton is
 well soaked, and very hot. It is then left to steep for twenty-
 four hours in soap-water. This process is usually performed
 between midnight and six in the morning; and the next morn-
 ing the fluid is drawn off, the cotton well washed and the
 water pressed out by a screw-press. This constitutes the first
 washing.

After the cotton has been well cleaned in this manner, the
 principal washing is effected as follows:

The washed cotton is disposed in the vessel as before, with
 this difference only, that the mass is enveloped in cotton cloth
 instead of linen, which is less capable of resisting the action of
 the caustic leys. A clear and colourless ley is prepared with



Salisbury
method of
bleaching cot-
ton thread, &c.

two thirds wood ashes and one third lime, and concentrate until an egg descends slowly in it (specific gravity about 1.081). This ley in a boiling state is poured upon the cotton; after which it is drawn off, again heated, and again poured on; and this repetition is continued from midnight to noon, or for twelve hours. The whole of the ley is then drawn off, and the cotton taken out of the vessel while hot, with the hands defended by gloves, and after spreading it upon a table, it is again put into the vessel, but in a reversed order. The same washing is repeated with other leys and continued for twenty-four hours.

Lastly, the boiler is filled with water, to which 2lbs. of soap are added, and the liquid brought to the boiling heat. The cotton is washed with this water, constantly kept boiling for twenty hours, and is afterwards left to steep for ten or eleven hours. The fluid is then drawn off, the cotton taken out of the vessel, and spread on a long wooden table, where it is washed and beated, and afterwards washed in a running water. Lastly, the water is pressed out by means of a press, and the cotton dried upon flaves in the sun, or in damp weather in a room appropriated to that purpose.

The remaining leys after these washings cannot be used a second time, but have a blood-red colour; they are mixed together, and are used in the common operations of the laundry. The soap waters are thrown away.

The flaves on which the cotton is suspended must be washed every time. It is preferable, when the weather permits, to suspend each piece by threads and stretched cords in the open air. The pieces ought not to be brought too near together, for fear lest the places to which the air has not free access should be spotted with brown spots.

Piece goods and raw linen thread will receive a partial bleaching by this method. It is necessary however to make the leys of only half the strength, and the lixiviation must be continued only half the time, for fear of weakening the texture.

Linen and cotton as vegetable substances have the defect of not taking fixed colours. The cause resides in their resinous principle, of which they must be deprived. Alcohol would be a sure and easy solvent for this principle, but it is too expensive. After alcohol, the best solvents of this principle are oils

oils of every description, but particularly fish oil. The cotton is soaked in this oil for 24 hours, after having again boiled it for two hours; it is then run and suspended in the air for some days, in order that the oil may be well separated, and it is entirely deprived of the oil by a strong hot ley with subsequent washing and drying. For this effect a ley of two thirds ashes and one third lime may be used, with the same quantity of sheep's dung as of both the before-mentioned ingredients. The cotton is not only boiled in this mixture, but it is also applied several successive times; the goods being washed in clear water between each lixiviation. The cotton thread may also be washed in water containing a sufficient quantity of potash to render it greasy to the feel. The thread is to be boiled for twelve hours in this ley, or till the fluid becomes black. Lastly, the thread is washed and steeped in water acidulated with weakly sulphuric acid, it is to be taken out of this fluid after remaining in it at least an hour, and then washed and dried.

If this method of bleaching were once generally known, it would not be necessary to purchase the article of the bleacher, or to send cotton to them to be bleached. In order to ascertain whether cotton bleached in any manner whatever is perfectly deprived of its resin, and proper to receive the dye, the glass is to be filled with water, and a thread of the cotton placed on the edge of the glass, so that half its length shall be within and half without. If the former end sinks in the water and the whole thread acts as a syphon, by causing it to run over, it will be a proof that the cotton will have the requisite purity.

The cotton thus purified must pass through three mordants: Red dye for
 1. A decoction of nut-galls. 2. A solution of tin; and 3. ^{cottons.} alum water.

For the decoction of nut-gall. For 1lb. of cotton or linen thread. For the former half a pound of the dark coloured nut-gall, or for the latter 1lb. is taken. The nut-gall is grossly pounded and boiled with a handful of birch leaves in a copper boiler, with two and half pots of rain water till reduced to one half. The liquid being left to settle and poured while yet hot through a cloth on the cotton, this is suffered to steep for 24 hours, frequently working or pressing, in order that it may be universally penetrated with the galling principle. It is then

**Red dye for
cottons.**

taken out, rung, and dried by suspending it in the open air, or in a heated chamber. It is needful only to take care that no water shall drop upon the cotton, as that would produce spots in the dye. In this operation the thread must be closely packed together, in order that the nut-gall may uniformly penetrate its mass. It is then conveyed into a solution of tin, made as hereafter described, and the decoction of nut-gall must be kept to be used in the aluming. The solution of tin is prepared in the following manner: Sal ammoniac or sea salt in fine powder is to be dissolved to saturation in 1lb. of aqua-fortis; 2 oz. of fine English tin rasped, are added to this, or as much of the metal as the acid can dissolve to saturation. In another vessel, 2 oz. of sea-salt are to be dissolved in one pint of rain water, and the solution of tin is to be poured drop by drop with continual agitation into this salt water. The galled thread is afterwards put into a stone ware vessel, closely pressed together, and the last mentioned mixture poured thereon. The thread must be occasionally compressed with the hand, in order to assist the penetration, and it must then be covered from the air and left to sleep for twenty-four hours, at the end of which time it is taken out, rung and dried in the air or in a heated room, where it may remain for 48 hours. After this it is washed in pure water again, dried, and emersed in the alum-water, of which we shall proceed to describe the composition.

The remaining solution of tin is reserved for a subsequent operation, for which, in that case, no more than three quarters of a pound of aqua-fortis and the other ingredients in proportion, need be taken for 1lb. of thread.

The alum water is made as follows: Whatever may be the kind of alum, it must be previously calcined. One pound of the crude alum is required for 1lb. of thread. When it is calcined it is pulverized and dissolved in one pint or English quart of water, and rather more than an equal measure of the remaining decoction of nut-gall is added. These being well mixed and heated, are to be poured on the thread, which has been treated as before with the solution of tin. It is left for fourteen hours in this bath and afterwards pressed and dyed.

The dying 1lb. of good bruised or ground madder previously soaked for some hours in water, is put into a boiler of sufficient size, which is to be filled with water and placed on

a moderate fire. As soon as the contents begin to heat, the thread is put in and continually turned. It is requisite that the thread should have been previously steeped in water, and afterwards wrung, in order that it may be very equally penetrated with the colour. The thread is left in the bath till the liquor boils, after which it is taken out, well washed, the threads are arranged on the hand, and the water pressed out. This first dyeing gives the thread a pale red colour. The boiler is then emptied and again filled with water, into which another pound of madder previously soaked in water is put, and this bath is heated. As soon as it has become hot, the thread is put in and boiled for one quarter of an hour. Care must be taken that the heat be not too strong. The first bath should scarcely boil, for fear the articles should take a brown colour; but this inconvenience is not to be feared in the second bath, when the thread is already penetrated with a red colour. After the thread is well washed for the second time, it ought to have a deep brown colour; if it has not it must be again boiled with a quarter of a pound of madder. After the last dye, the thread is to be washed by hand in hot soap water, after having washed it in cold water, and it is then to be dried.

Red dye for
cottons.

XV.

Report on an artificial Production of Camphor, announced by
M. KIND. *Read to the Society of Pharmacy by* BOULLAY.*

BEING charged, in conjunction with M. M. Cluzel and Chomet, to give an account to the Society of an interesting phenomenon announced by M. Kind, in *Trommsdorff's Journal de Pharmacie*, extracted and reported in the 14th number of the *Journal de Physique et de Chimie*, published by Van Mons, I now inform them of the result of our labours.

"For the purpose of procuring the arthritic liquor of Pott, which is prescribed here with advantage, for resolving the nodosities and other obstinate tumours which form upon the joints, I intended," says M. Kind, "to incorporate the muriatic gas

M. Kind's
experiment of
the production
of camphor by
common oil of
turpentine to
absorb muriatic
gas.

* From *Annales de Chimie*, *Fruktidor*, No. 153. Vol. LI.

with oil of turpentine, at the instant of the disengagement of the gas. This operation shewed me a phenomenon which, I am of opinion, is deserving of being known.

"I put decrepitated muriate of soda into a tubulated retort; I adapted two of Woulf's bottles to the retort, into which I had put a quantity of oil of turpentine, equal in weight to the salt. On this I poured half the proportion of concentrated sulphuric acid, and I disengaged the muriatic gas by a graduated fire. At first the oil acquired a yellow colour, afterwards a pale brown colour, which, towards the end turned to a deep brown. After cooling, the liquor was almost wholly coagulated into a crystalline mass, which, in every respect, comported itself like camphor.

"I do not think," says M. Kind, "that a similar production of camphor has ever been noticed before; but Meyer speaks of a concretion of a camphoric nature, formed in oil of turpentine digested with dry caustic salt." He afterwards instances a formation of camphor observed by the late M. Trommsdorff, on distilling essential oils on lime. "It is probable," says M. Kind, "that the great tendency of muriatic gas to combine with water, determined the union of the principles of this liquid contained in the oil, so that the carbon became predominant, and made the oil brown; and, that the effect of this acid gas on it, corresponded with that observed by Achard, in the action of concentrated sulphuric acid on fat oils. He did not remark that the heat was as considerable during the action of the gas as Woulf stated it to be."

Repeated by
M. Trommsdorff.

In an additional note, M. Trommsdorff announces that he had repeated M. Kind's experiment, and that he had obtained analogous results.

He dried the concrete matter between sheets of paper; it became very white, semi-transparent, and possessed the following properties:

"Its odour was strong, very analogous to that of camphor, but injured by that of the turpentine.

"Placed in a silver spoon, and heated over red hot coals, it volatilized without leaving any residue, and yielding a strong smell of camphor.

"Its vapour inflamed.

"Exposed to a gentle heat, in a small phial, it was entirely sublimed. It dissolved with facility in oil of almonds.

"It was entirely dissolved by alcohol, but not so slowly as common camphor, and was precipitated unaltered by water.

"Concentrated nitric acid affected its solution, at first tranquilly, but afterwards with a disengagement of nitrous gas and heat. The mixture became turbid by the addition of water, but did not deposit camphor."

He concludes from these experiments that this product comported itself exactly like camphor, except with the nitric acid; but he believes that this difference may depend on a small quantity of muriatic acid, or a little oil of turpentine, from which it is very difficult to free it entirely.

M. Trommsdorff afterwards disengaged the gas from a quantity of muriate of soda equal to that employed in the first experiment, through the matter remaining in the bottle; but he found that instead of augmenting, the camphorized part diminished in quantity, lost its camphoric odour, and the alteration of its other properties induced this chemist to believe that the new addition of gas had caused it to undergo a decomposition.

The above experiments, repeated with the greatest attention, gave us satisfactory results, even with the different species of oil of turpentine of commerce.

Repetition of these experiments by the reports.

The importance of the subject, and the objections made by some members, at the reading of the observation which lead to this report, made us sensible how much the labours of M. Kind left to be wished for; the quantities of the product also not being indicated, we thought we should accomplish the intention of the Society, in following the track marked out by the author, if we endeavoured to add some facts to those he has published, and we proposed the following questions for our guidance.

1st. Does the gaseous muriatic acid serve to determine the reaction of the principles of the oil of turpentine, and to change their proportion, to form the camphor; or, is its action limited to insulating this immediate principle, from the oil with which it was not combined?

Questions for investigation.

2d. What is the quantity of the camphoric product?

3rd. Will a greater quantity of the gas than that indicated by M. Kind add to the product, or will it destroy it?

4th. Will another quantity of gas added to the liquor which has yielded the camphor, and from which it has been separated, determine a new formation of this principle in it?

5th. What

5th. What are the best means of purifying this camphor, and of freeing it from the odour which it retains?

6th. What are the relations and differences between the purified camphor of turpentine, and that of the camphor-laurel?

7th. Would liquid muriatic acid produce the same effect?

8th. Would the other acids act in the same manner on this oily substance?

The complete solution of these different questions would doubtless have required a great number of experiments, to which we were unable to attend: we shall only relate those which we made.

Apparatus.

With a view to obtain a more considerable product, which would yield us the concrete matter in a quantity sufficient to be ascertained and subjected to different experiments, we prepared an apparatus similar to that used by the author of the discovery, but of larger dimensions. It consisted of a tubulated retort, and two Woulf's bottles, provided with their tubes of communication and safety. Four pounds of marine salt, and two pounds of concentrated sulphuric acid were introduced into the retort; four pounds of very white oil of turpentine were poured into the first bottle, and two pounds of distilled water into the second. Fire was placed under the mixture, and augmented gradually till the entire disengagement of the muriatic acid gas, which was totally absorbed by the oil. The latter exhibited the following phenomena: it became at first of a citron colour, and afterwards brownish, which grew gradually darker towards the end of the operation; it retained its transparency; it grew very hot; its volume augmented about a tenth; the sides of the bottle were covered with small limpid drops which united slowly at its bottom, in the form of a thick oil.

Proportion of ingredients.

All the gas was absorbed. Changes of the oil.

Crystalline product by repose.

Left to itself for twenty-four hours, it formed a mass of irregular crystals, from which ran a brown liquid. When dried and strongly pressed between sheets of paper, this crystalline matter was very white, and of a peculiar odour, participating of the oil and the acid made use of; it weighed twenty-four ounces.

More crystals by cold.

The liquor which had been separated from these crystals, exposed in a cellar for several days, yielded others, which dried like the first, were of equal whiteness, and weighed four ounces.

To ascertain whether there did not yet exist a further quantity of this concrete matter ready formed, in this sort of mother-water of camphor, it was exposed to an artificial cold of eight or ten degrees below 0, during several hours. By this means it yielded other crystals, which, collected like the preceding, offered the same characters, and weighed two ounces; this brings the quantity of camphor to seven ounces and a half for each pound of oil of turpentine.

This liquor, not appearing capable of yielding more crystals, was divided into two portions, one of which was reserved for examination in this state, the other was subjected to the action of a new quantity of acid gas; it dissolved a part of it, and became almost black, without losing its transparence, even after remaining for several days in the cellar.

Another dose of oil of turpentine was charged with a portion of gas, double that mentioned above. The precipitation of camphor took place in nearly a similar quantity; the liquor was very little more coloured, but a much greater quantity of fat oil was formed.

An ounce of this matter washed in distilled water, and put in a filter to drain, became of the most beautiful white; it no longer gave signs of acidity, but had still an odour of turpentine. The water in which it had been washed was very aromatic; it reddened the tincture of turnsole, and precipitated by nitrate of silver.

An equal quantity of the same substance was washed with water impregnated with a fiftieth part of unsaturated carbonate of potash: it lost much of its first odour.

New portions of this rough camphor were mixed in equal parts, one with charcoal in powder, another with very dry ashes, a third with quick-lime, a fourth with alumine (*porcelain earth*), and were introduced separately into small glass alembics, the capitals of which were, by a gentle heat, rapidly covered with distinct groupes, composed of a multitude of small needle-formed crystals, diverging from the same center.

Purified in this manner, it entirely lost the odour of oil of turpentine; that which it retained was more analogous to common camphor, but not so strong. In this state it swims on water, to which it communicates its taste; it burns on its surface, without experiencing the revolving motion observed by

Professor

Professor Venturi. The only motion we observed was the result of the molecular attraction, which determined a great number of the fragments dispersed over the surface of the water to collect in one point.

Is soluble in alcohol.

It is wholly and readily dissolved in alcohol, from which water separates it unaltered.

Action of dilute nitric acid.

Nitric acid, at thirty degrees of *Beaumé's* areometer, poured on this new camphor, had no action on it, even after several days of contact; although the same acid dissolves the common camphor with the greatest facility, becoming covered with an oily stratum, and water precipitates pure camphor from it.

Of concentrated nitric acid.

Very concentrated nitric acid had not, at first, any action on our camphor; but after a few seconds the solution is effected, and at the same time a disengagement of nitrous gas is manifested: water rendered this solution very slightly turbid. The same acid, on the contrary, dissolved the camphor of the shops rapidly, diffusing white vapours. This solution took a beautiful fire-red colour, and water separated pure camphor from it in flocks.

It is not dissolved by acetic acid.

Neither did acetic acid, which perfectly dissolves the common camphor, effect the solution of this. By heat, it softened and seemed to dissolve; but on cooling, it was wholly collected on the surface of the liquid, with all its properties.

We afterwards examined the liquor called the *mother-water of camphor*.

Physical properties of the mother-water. Peculiar oil.

This brown transparent liquor, lighter than water, heavier than alcohol, fuming by contact of the air, of a peculiar odour, swam above a thick reddish oil, the small quantity of which obliged us to neglect the examination of it.* The taste of this mother-water was sharp; it did not mix with water, nor yield its acid to it. Agitated in this liquid, its particles remained dispersed for a long time, before they collected on its surface.

Soluble in alcohol and ether.

Alcohol and ether dissolved it completely; but the addition of water separated them.

Inflammable.

Inflamed in a capsule, it burns without a residue, diffusing a considerable quantity of very odorous smoke.

Action of sulphuric acid.

Concentrated sulphuric acid poured into this liquor, blackened it, and disengaged from it some vapours, and a very strong odour of gaseous muriatic acid.

* We only ascertained its solubility in alcohol and sulphuric ether, and its insolubility in water.

A solution

A solution of crystallized carbonate of soda produced a slight effervescence in it, discoloured it, rendered it lighter, and formed muriate of soda. and of carbonate of soda.

This muriated oil, distilled to dryness in a suitable apparatus, yielded some muriatic gas, some drops of thick oil, similar to that mentioned above, a very light and very combustible perfumed liquid, which comported itself with water, alcohol, sulphuric acid, and soda, in the same manner as before distillation; there was only less muriatic acid in it. The residue was a black matter, glutinous like pitch, dissolving imperfectly in alcohol, and yielding muriatic acid to water. Products by its distillation.

We afterwards made a mixture of equal parts of oil of turpentine and muriatic acid at 25°. We agitated it from time to time, and then left it for several days. The oil took a reddish brown colour, the acid also became reddish: both of them preserved their transparency, and at the point of contact of these two liquids, was perceived a concrete stratum, having several of the properties of camphor, but its quantity was so small that we were obliged to neglect it. Mutual action of oil of turpentine and muriatic acid produces a small quantity of a concrete body.

A fact of this kind is to be met with in the translation of the Chemical Recreations of *Model*, published in 1774, by *M. Parmentier*. Previous discovery of this fact.

This philosopher expresses himself as follows, in page 400 of the first volume of this translation, in the additions which he has made to it:

“A very curious and very interesting observation to natural philosophy and chemistry, is, that *M. Merges*, surgeon, in a work, the object of which is the examination and chemical analysis of different medicaments *, says, that he obtained, from the digestion of a mixture of fuming marine acid and oil of turpentine, small saline concretions, which at the end of a certain time, became more considerable, and took the figure of a crystalline salt, in form of a parallelopipedon, while the oil which swam above them was coloured red, and acquired a very thick consistence, &c.

It is very probable that these crystals were the same as those which were obtained in such great abundance from the action of the same gaseous acid.

The same oil was saturated with oxygenated muriatic acid Oxygenated muriatic acid gas and oil of turpentine.

* Paris, 1774. Second edition.

No camphor.

gas; it acquired a deep yellow, retained its transparency, became a little more consistent, but did not produce any camphor.

Action of sulphuric acid.

Concentrated sulphuric acid acting too violently on oil of turpentine, the decomposition of which it speedily effects, we made a mixture of equal weights of this oil and sulphuric acid marking only 40° of the areometer. The action was confined to the point of contact, and was analogous to that of liquid muriatic acid.

Slow distillation of oil of turpentine.

Eight ounces of oil of turpentine were kept for three days in a glass alembic, placed on a sand-bath, the temperature of which was maintained at about forty degrees of *Reaumur's* scale. This time being elapsed, we found in the recipient nearly four ounces of a very white and very light volatile oil, and in the capital many small crystallizations of true camphor. The oil remaining in the cucurbit was of an amber colour, and very much thickened.

Action of muriatic acid gas on the other volatile oils.

To satisfy ourselves whether the other volatile oils would furnish camphor by the action of the muriatic gas, we incorporated them with it in different proportions; but did not obtain any precipitation. They became very black, and contained a great quantity of heavy oil, &c.

CONCLUSION.

General results. 1st. That the production of camphor, announced by M. *Kind*, is uniform in the proportions he indicates, and of whatever soil the oil of turpentine may be.

2nd. That the camphoric product may be estimated at about half the quantity of the oil employed.

3rd. That a greater quantity of the gas does not add to the product, nor does it destroy it.

4th. That this camphor may be purified by water, by alkaline solutions, charcoal ashes, quick-lime and alumine: and, that these three last substances are more especially preferable.

5th. That it differs from what is extracted from the camphor-laurel, in its taste, which is not so bitter, and in its odour, which is less penetrating; and, that it presents an absolute difference in its mode of action with the nitric and acetic acids, the first of which only dissolves it by a reciprocal decomposition.

6th. That, notwithstanding these differences, it is not the less a beautiful new species of this immediate principle of vegetables to chemists; and, that it is to be wished that physicians would try it, to ascertain whether, as there is reason to believe; its action will be the same on the animal economy.

7th. That, during the operation from which this camphor results, there is formed a true combination of muriatic acid and volatile oil, having a resemblance to what the ancients called *acid soaps*.

8th. That liquid muriatic acid, and dilute sulphuric acid, not mixing with oil of turpentine, their action is confined to their point of contact.

9th. That the mode of action of the muriatic gas, in this circumstance, may be explained by the elegant theory which MM. Fourcroy and Vauquelin have given of the action of sulphuric acid on vegetable substances in general.

10th. That, notwithstanding camphor is set free by the slow distillation of oil of turpentine, without addition, we are led to believe that so large a quantity of this matter could not exist ready formed in it, and that at least a part is the result of the re-action of the principles of the oil, excited by the presence of the acid gas, and its affinity for water.

11th. Finally, that the same process is not applicable to the extraction of the camphor contained in the volatile oils of lavender and rosemary.

XVI.

Letter from JOSEPH HUDDART, Esq. F. R. S. on the apparent
Enlargement of the Moon at low Altitudes.

To Mr. NICHOLSON.

DEAR SIR,

IT was not until yesterday that I received your Journals, Introduction, Nos 36 and 37: In the former, reading a letter from C. L. occasioned my referring to No. 35, page 164, of the Journal, to Mr. Walker's letter respecting the sensible magnitude of the
the

the horizontal moon above what it has in the zenith; and therefore present you with the idea I have entertained upon the subject.

Estimate of magnitude confined to the size of the image on the retina.

I am of opinion that the magnitude of any object can only be estimated (as to sense) according to the space or magnitude of its image upon the retina. The eye must (if this is admitted), therefore, be the sole cause of the illusion, for the moon's apparent magnitude increases as she rises towards the zenith.

The eye is formed not only to alter its aperture, but also its focal distance.

Elongation of the eye will enlarge the image, though the angular magnitude remains unaltered.

The eye by the power of the muscles can, at the will of the observer, not only be directed towards the object, in order to receive its image upon the usual part of the retina, but acquire a proper convexity for distinct vision, or focus of the refracted rays upon the retina; and also to contract or extend the area of the pupil according to the quantity of light. This admirable property may be observed by every one in passing out of a light room in the night, and also in the eyes of the brute creation; but I conceive, that while the area of the pupil is enlarged in order to receive a greater quantity of light, the eye is protruded, or the focal distance between the pupil and retina is increased, which increases the sensible magnitude of the object, as the image upon the retina conveys it to our senses, when viewed by the naked eye. This I distinguish from apparent magnitude, which we use when actually measured by an instrument.

Variation of the pupil, or of the aperture of a lens, cannot alter the image.

The elongation of the eye is supposed to take place whenever the pupil is enlarged;

The variation in the magnitude of the image upon the retina, certainly cannot arise from the variations in the dimensions of the pupil, which is contrary to the law of dioptrics; for, on observing the sun, I do not admit light through one-twentieth part of the area of the object glass, which is 3.8 inches in diameter, and yet there is not any difference in the apparent diameter; or, whether the rays are admitted at the center or any other part of the object-glass, the observations are equally good. But I consider it as a natural consequence, that when the pupil of the eye is enlarged, the focal distance is increased, and which constantly arises from a diminution of light, even without our attention; but thereby our sight is assisted in consequence with more light and power, which is some compensation.

and as this takes place when the moon is least lu-

The light from a celestial object near the horizon, is diminished by passing a long distance through a gross atmosphere;

hence a sensible enlargement takes place in the moon as well as other bodies, which diminishes as the altitude increases, and the light less interrupted. minous, it then appears larger.

I am, Sir,

Your most obedient,

J. HUDDART.

Highbury Terrace, Jun. 28, 1805.

XVII.

A Memoir on Milk and the Lactic Acid. By Cit. BOUILLON LAGRANGE*.

SECTION I.

The present State of our Knowledge respecting the Lactic Acid.

CHEELE and Deyeux, and Parmentier, are the chemists who have most contributed to our knowledge of the acid of milk. History of experiments on milk, and the authors of discoveries, &c.

The experiments made before the time of these skilful philosophers, are those of Hoffman, Boerhaave, Homberg, Geoffroy, Rouelle the younger, and Baumé. But the analysis of animal matters was then in such an imperfect state, that little can be gathered from their experiments.

Scheele directed his labours with very different views. His well conducted experiments led him to determine the characters and properties of an acid afforded by the serum of milk. But not withstanding the labours of this celebrated man, we have some subjects of inquiry left for examination.

Besides the great number of its saline compounds, as Fourcroy remarks, which are still wanting to the science, Lehale has Imperfect state of our knowledge of the lactic acid.

* *Annales de Chemie*, No. 150. vol. 50.

The author in a note mentions his having learned on the evening when his Memoir was read, that Messrs. Vauquelin and Thénard had each been separately employed on the same subject: and on that occasion he then finds it necessary to say, that most of the experiments he describes, are the result of observations made by several pharmacians, at the sitting of the society of pharmacy, the 15 Nivôse, in the year 12. The verbal procès of that day, signed by Parmentier, president, and Delunel, secretary, of which an extract was sent to the Philomathic Society, ascertains the experiments he had then made, and the observations of the several members of the society of Pharmacy.

not

not pointed out the action of fire upon this acid, nor its spontaneous alteration in the air, the manner in which it comports itself with the nitric acid, &c. It is not known whether it be totally decomposed by this last, or converted into some other acid, particularly the oxalic acid. We are entirely unacquainted with the nature of its composition. Though it presents properties which resemble those of the acetic acid, and lead to the probability that it nearly resembles it; we cannot yet rank it with the vegetable acids. On the other hand, nothing decisive can be asserted with respect to its animal nature, because no experiment has yet indicated the presence of azote, and it is yet unknown, whether it may afford ammonia in its decomposition; whether it be putrescible, or convertible into prussic acid, &c.

This general statement while it gives us a view of our knowledge of the lactic acid traces, as it were, the steps necessary to be pursued in determining the place which this acid ought to occupy, as to the number of well established chemical facts.

SECTION II.

Concerning Milk and the caseous Matter.

On milk, and its constituent parts

Before I proceed to describe the experiments I have made on this article, it will be useful to present some reflections on milk, and its constituent parts.

Gutton, in the *Encyclopédie Methodique*, offers two questions respecting the existence of an acid in milk. He expresses himself thus :

Whether whey exists ready formed in milk.

Does the whey exist in milk such as it is found after the separation of the other constituent parts? Does it manifest acid properties on any other account, than because it holds salts in solution, as all the analyses suppose?

Milk cannot be reproduced by the mixture of its parts.

If whey, adds the same chemist, existed in milk, in the state it exhibits after the separation of the butter and cheese, we should be able to reproduce milk by mixing these three principles again in the same proportions. But as this is not the case, he concludes, that whey is the product of a true fermentation.

The examination of this first question founded on experiment, ought, I think, to prove that it will not be sufficient to shew whether whey exists in milk, that we should mix the three principles in the same proportions, and re-produce that fluid; first, because these principles are no longer the same; and

and secondly, because whey formed by the usual process, is more or less acid.

Experiment 1. Recent milk gives a red colour to the paper and the tincture of turnsole. Exp. 1. Milk reddens turnsole.

Experiment 2. If milk be distilled in close vessels, and the product separately taken, the first is not acid; the second reddens turnsole, and slightly precipitates the nitrate of silver; and the third has no action on the tincture. At this period, the milk is not yet decomposed, but still reddens turnsole. It appears that the remaining acid is no longer volatile, but is retained either by the animal matter, or by some other substance. Exp. 2. It affords acid by distillation, and continues acid.

Experiment 3. If milk be coagulated with a mineral or vegetable acid, the whey is separated, without retaining a particle of the acid made use of. Exp. 3. Whey of milk coagulated by an acid retains none of the acid itself.

I must however observe, that some deception would follow, if the serum only were examined, which is obtained by the sulphuric acid, or by allum, because the precipitate afforded by barytes, is not entirely re-dissolved by the nitric acid. I am satisfied that this arises only from a small quantity of sulphurate of potash contained in the serum, as is evidently proved, when the whey is separated by any other substance not of an acid nature.

It is not true therefore as many chemists have advanced, that the acid separates the caseous matter, by uniting with the serum. Exp. 4. Whey from milk spontaneously decomposed, is also acid.

Experiment 4. If milk be exposed to the air at the temperature between 12 and 20 degrees (about 66° Fahrenheit) the separation takes place in twenty-four hours. The curd has a more acid flavour than that of experiment 3. Hot water causes it to lose its sour taste, and acquires the property of reddening turnsole.

We here find nearly the same properties in the cheesy matter, when spontaneously formed, and when separated by acids.

Experiment 5. The same experiment being made with the pneumatic apparatus, was attended with no absorption of air, or disengagement of elastic fluid. The only difference was, that the separation of the cheesy matter did not take place for several days. Part floated on the liquid, and the other occupied the bottom of the vessel. Exp. 5. Milk spontaneously decomposed, neither absorbs nor gives out air.

Experiment 6. A bottle was filled with fresh milk, and well corked. Some days afterwards, the cheesy matter had separated. As soon as the separation was complete, the cork was intended to be extracted, but it flew out with violence and noise. Exp. 6. Milk decomposed spont in a closed bottle, gives carbonic acid.

noise, indicating the presence of an elastic fluid. Upon shaking the bottle, a greater quantity of gas was disengaged, which being collected and examined, had the same characters as carbonic acid.

The fluid as well as the curd had a sharp acid taste, which became less pungent, as the carbonic acid was disengaged by agitation.

The fluid, after having been heated, did not appear to be more acid than serum, obtained by exposing milk for the same time to the air.

The same experiment was made with milk which had been boiled about half an hour, and the results were the same.

**Qual-
ity of the
Cheesy matters.**

Experiment 7. The cheesy matters of the third, fourth, and fifth experiments, differ essentially in taste and consistence.

That of No. 3. is dry and firm; that of No. 4. is more divided, less dry, and partly soluble in water; and lastly, that obtained in the experiments No. 5 and 6. is not clotted, but is more light, and does not unite into a mass, till after several hours.

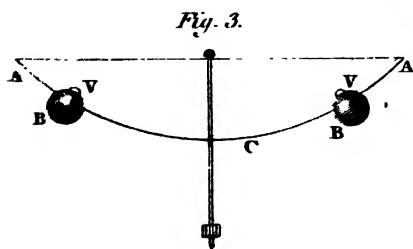
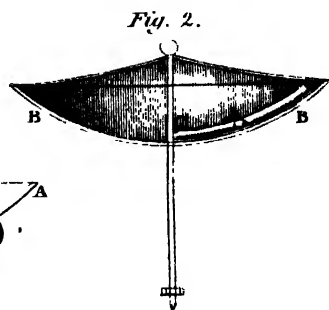
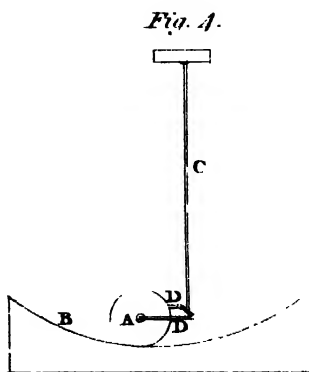
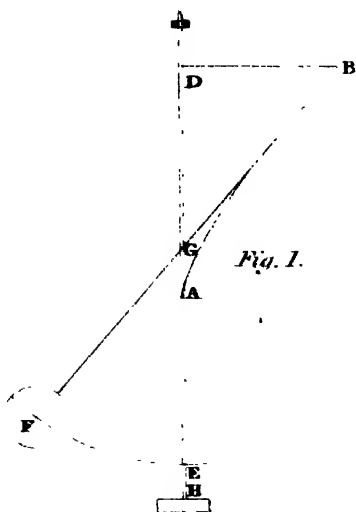
The cheesy matter, therefore, requires particular properties, according to the substances and the processes employed to separate the serum.

**Deductions from
Expts.**

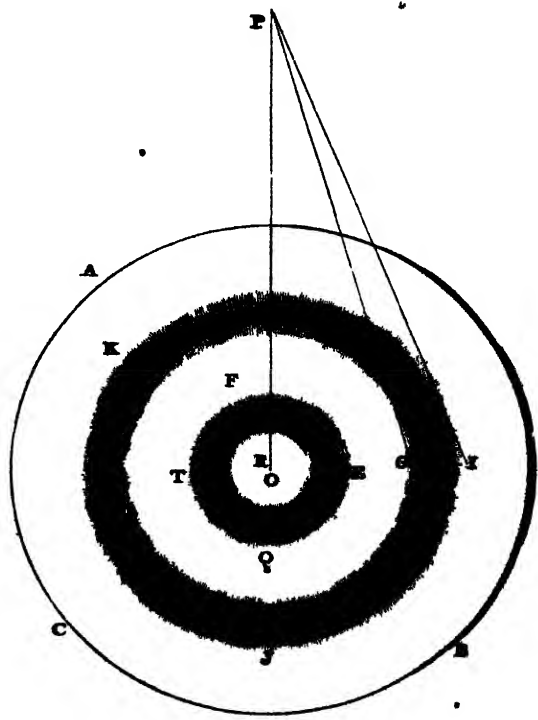
These first experiments lead us to the following reflections, that, 1. Milk does not require to be decomposed, in order to manifest the presence of an acid. 2. That this acid is mixed with salts, sugar, and animal matter. 3. That the acid in milk is disengaged, though it is not very perceptible, but by re-agents. 4. That the contact of the air is not necessary for separating the constituent parts of milk. 5. That the coagulating substances merely facilitate the separation of the cheese, either by forming a new compound, or by more immediately condensing the particles together, when the cheesy matter requires new properties: or lastly, by exposing the milk to the air. By degrees, a part of the sugar of milk is decomposed; carbonic acid is formed, of which one part is disengaged, and the other facilitates the separation of the curd; caloric also favours the attractions of coagulating matters for the curd. They all, differently, for alcohol, which also possesses this property, affords other results; the curd being to a certain point soluble in water. We cannot therefore as chemists have attributed these effects to the solution of these substances in water, and their greater attraction for the liquid than is exerted by the cheesy matter itself.

(To be continued.)

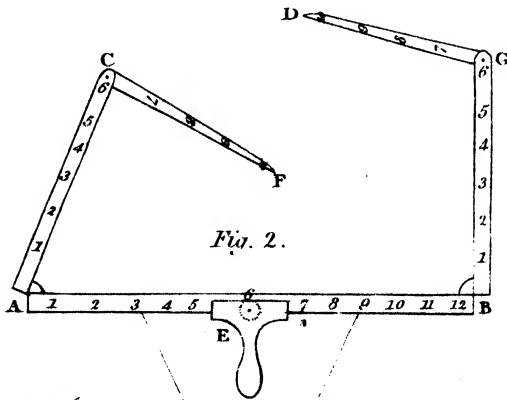
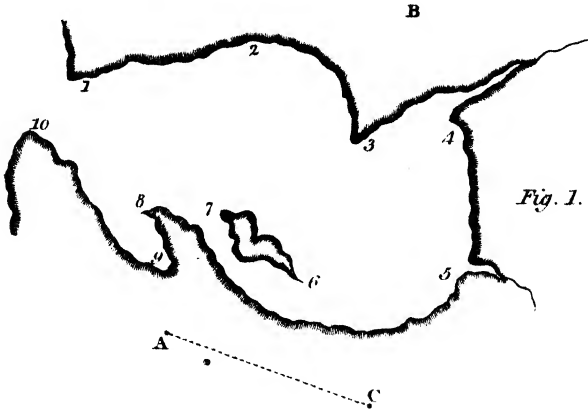
Outlines of Pendulous measures of time
By J. Whitley Borswell Esq.



*M^r Gough's theory of the Augmentations
of Sound by communicated vibrations.*



*Capt. Mortlock's Method
of surveying, by very simple Instruments.*



*Instrument
Designed from
for taking
Nature.*

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MARCH, 1805.

ARTICLE I.

Historical and Critical Observations relating chiefly to the Invention of the Telescope. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

ALTHOUGH I was aware that the experiment on the in- Incompressibility of water.
compressibility of water was to be found in Lord Bacon's work, yet I was happy to see that Sir H. Englefield had taken the trouble of reminding your readers of the original inventor of it. I am always glad when I see any thing which may recall our attention to the works of our illustrious countryman. They abound with instruction, and, in many instances, his conjectures may be compared to the Queries of Sir I. Newton, which contain the foundations of the greatest discoveries.

In the same paper Sir H. Englefield has given us a very curious passage from Kepler; and although I cannot persuade myself that he is accurate in his interpretation of it; yet I think the argument has been by no means accurately stated by Aletes*. I shall therefore beg your permission to insert some additional remarks on a subject, which certainly possesses a considerable share of interest. I must beg leave, likewise,

Remarks on the communications of Sir H. C. Englefield and Aletes.

* Vol. X. p. 92.

Observations
relating to the
invention of the
telescope.

previously to remark that Sir H. Englefield has not asserted that Kepler was the original inventor of the telescope. The only inference which can be justly drawn from his words, is that they were invented as early as the year 1607. No one, indeed, who had attentively read Kepler's dedication of his *Dioptrice*, could have entertained such an opinion. He precisely states* that since "ad magnum cumulum inventionum hujus ultimi sæculi accessisset *Arundo Dioptrica*, nequaquam inter vulgares commemoranda machinationes, circaque eam, alii de palma primæ inventionis certarent, alii de perfectione instrumenti se jactarent, . . . Galilæus vero super usu patetfacto in perquirendis arcanis Astronomicis speciosissimum triumphum ageret; . . . Ego ductus honesta quadam æmulatione novum mathematicis campum aperui . . . causarum lege geometrica demonstrandarum quibus . . . effectus inniterentur."

Before the invention of telescopes, astronomical observations were sometimes made with tubes, but more generally with rods (*regulæ*) which had sights fixed at each end. These instruments were generally called *Dioptra*, and the apertures in the sights were called *foramina dioptrorum*, not *perspicilla*. Ptolemy in his *Almegist* † describes the instrument, which is generally known by the name of his *Triquetrum*. It had sights at each end, with apertures in them, which Georgius Trapezuntius, the translator, calls *foramina*: he mentions, indeed, no particular name for this part of the instrument, but Theon in his commentary ‡ describes the construction of it to be such, that the whole moon would appear through the sights *εν τῇ διοπτρῳ*. Proclus Diadochus, in his *Hypotyposis Astronomicarum Positionum*,** mentions the *Dioptra* of Hipparchus, which was an instrument of the same kind: see Riccioli *Almegistum Novum*, vol. I. B. 3. C. 10. § 4. But we can have no further doubt upon the subject, if we turn to Flamsteed's *Historia Cælestis*, vol. III. p. 97. He there says that "haud pauci

* Pp. 53, 54. Lond. 1633.

† Book 5, chap. 12.

‡ P. 258. Basil, 1538. Notwithstanding what Harduinus says, I have no doubt, but that the *Dioptra* mentioned by Pliny in his *Natural History*, B. 2 C. 69. was an instrument of this kind; but no description of it is given, and therefore I cannot argue upon it in this place.

** P. 399. Basil, 1541.

observationum Tychonicarum errores ex nudorum Dioptrorum usu necessarie consequuntur." Now it is only necessary to examine the Astronomiæ Instauratæ Mechanica, and we shall see that almost all the instruments, which Tycho Brahe used, are fitted with sights, which were some perforated with small holes, and some divided by narrow slits: this part of the instrument he called *dioptra*, the perforations he called *foramina*, and the slits *rimulæ*.

I could quote further proof that these words were commonly used in the senses which I have affixed to them, but it would be not only unnecessary but tedious. It must, therefore, remain with Aletes to produce as good authorities for the alleged difference between *perspicillum* and *perspicillum vitreum*. I cannot help thinking, however, that the very title-page of Kepler's Dioptrice will be fatal to his explanation; for there we find that mention is made of the discoveries of Galilæo, made "*ope perspicilli*" after the publication of the Siderius Nuntius. Here the word *perspicillum* is undoubtedly used for the telescope itself, and before I conclude what I have to say, I shall quote a passage from Galilæo, which is still more contradictory to the ideas of Aletes.

Although I differ from your correspondent with respect to the arguments, which he has stated to you, still I agree with him in his general conclusion, that Kepler did not mean a telescope by the word *perspicilla*. We learn, indeed, from Borelli's book de vero inventore telescopii, that * one account makes the invention as early as the year 1590; but when we compare this with the deposition of Sara Gædarda † and what is stated by Galilæo in (p. 10 of) his Sidereus Nuntius, it seems most probable that telescopes were not known before the year 1609. If Zachary Jausens was acquainted with them before that period, he seems not to have published his discovery to the world. Now Kepler, in the place above quoted from the dedication of his Dioptrice, takes an opportunity of praising the invention in the highest terms, and as he did this, when it was public, it is probable that he would have done so still more warmly, if he had been the first who published an account of observations made with them; but there is nothing of this kind in the passage alluded to in his book de cometis.

* P. 25. † p. 31. This makes the invention about 1611 or 1613, but "de certo præfixo tempore non potuit dicere."

Observations
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telescope.

He was looking at the stars with a friend on the bridge at Prague, and they observed one in particular. "Vidi"* he says, "*stellam sub urſa, majorem cæteris per perſpicilla intuitus, quæ æquale cæteris fixis lumen mihi ſine perſpicillis diffundere videbatur.*" It is not likely that the perſon, who mentioned the firſt uſe of ſuch an inſtrument as a telescope, would confine himſelf to ſo dry an account of the effects produced by it. But there is another argument, which may be drawn from the book itſelf, and that is, when Kepler is giving an account † of his obſerving a comet in the year 1618, he uſes the word *teſcopium* and not *perſpicillum*. "6 Sept. jam nulla amplius cunda viſu nudo potuit obſervari et *teſcopio inſpectus ſatis magnus apparuit.*" "Sept. 23. Planitudo major ſine *teſcopio*, quam per illud." ‡

But what puts it beyond doubt that Kepler was not in poſſeſſion of a telescope ſo early as the year 1607, is that we hear of no great diſcoveries which he made about that time by obſerving the heavens. Monſieur de Moutucla, indeed, ſays § that "*Uniquement appliqué a determiner avec preciſion les mouvemens celeſtes, cet homme celebre faiſoit peu d'uſage du telescope.*" He probably thought himſelf juſtified in this, when others were employed in the more mechanical labour of obſervation. When he had no ſuch fellow-labourers, it is impoſſible that a man like Kepler could have neglected an opportunity of obſerving, or have concealed the means of doing ſo; and if he had not done both, we muſt have had ſome notices remaining either in his own writings or thoſe of others. — We muſt recollect likewiſe, that he was the firſt who underſtood the theory of telescopes and the combination of lenſes, and we muſt not forget, that theſe diſcoveries being once made, the greateſt difficulty was overcome; it was only neceſſary to uſe the telescope and mark the objects which it preſented to the ſight.

From what I have ſtated there ſeems to be conſiderable difficulties attending either of the ways, which have been propoſed for interpreting the word *perſpicilla*. But there are ſome paſſages in the *Sidereus Nuntius*, which ſeem to aſſiſt us in affixing a meaning to the word, leſs exceptionable than the one hazarded by Aletes.

* 16 Sept. 1607.
26

† P. 48.

‡ P. 53.

§ *Hiſtoire des Mathematiques*, vol. II. p. 234.

In p. 11. Galilæo says "tubum primo plumbeum mihi paravi, in cuius extremitates vitrea duo perspicilla . . . aptavi." ^{Observations relating to the invention of the telescope.} Again in p. 12. he says "dum nulla in tubo adessent perspicilla," the rays pass in one direction, but "appositis perspicillis," they will pass in another. Hence it appears that Galilæo makes no difference between *perspicilla* and *vitrea perspicilla*, but uses both indiscriminately for the glasses of his telescopes. I am inclined indeed to believe that the original meaning of *perspicillum* was like that of *conspicillum* a mere lens; that when lenses were combined in telescopes it was used both for the glass and the instrument, until the word *telescopium* became common. I am not, however, prepared to bring forward sufficient authorities for this account of the word, and I have not leisure at present to search for them. It is sufficient for my purpose that we are justified in understanding the word as meaning a mere lens; for if we consider it as such in the passage under consideration all the difficulty vanishes.— Kepler complains in this very book *de cometis* * of the weakness of his sight; he might, therefore, have probably used a simple lens to assist it. Monsieur Dutens † has clearly proved that the convex lens was known to the ancients, and at the same time ‡ that Borelli calls Zachary Jansens, "conspicilliorum artifex peritissimus," he speaks of his concave as well as convex glasses.

There is one circumstance attending the observation itself, which seems to give some support to my interpretation, which is, that it was not made at a time when Kepler was carefully examining the heavens for any new phenomena; but in a walk which he accidentally took with his friend. Now he may easily be supposed to have carried about with him a simple lens to correct any fault of his eyes, but it is scarcely probable that he would have had with him a telescope, made, possibly like Galilæo's, of a long leaden pipe. I scarcely need add, that whatever clears and strengthens the sight will diminish the apparent diameter of a fixed star, and consequently increase the relative magnitude and light of the comet.

* P. 30. *Instrumenta Tychonica, &c. &c. et oculos vegetos requirunt, quæ omnia mihi defuerunt*, 1607, and in p. 48. *Debili sum visu*, 1618.

† *Decouvertes des anciens. Partie 3, chap. 10. §. 278.* ‡ P. 25.

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relating to the
invention of the
telescope.

I am conscious, Sir, that what I now send you, is put together in a very hasty manner. Were my time at my own disposal, I would endeavour to make it more fit to meet the public eye; but I am induced to send you these hints in their present state, because I rely as much upon the candour as on the learning and abilities of Sir H. Englefield. If I am mistaken, he will set me right; and if I have any foundation for my opinion, he can easily illustrate my hypothesis by the passages which agree with it, in the old writers on optics and astronomy. I am, Sir,

Your obliged humble servant,

E. O.

Oxford, Feb. 10, 1805.

P. S. Since I finished the letter, which I sent to you on the 10th inst. I have met with a passage, which appears to me to decide the question on which I wrote to you. It is in Kepler's book, which he published at Francfort in 1604, under the title of *Astronomiæ pars optica*. The beginning of it is entitled *Paralipomena in Vitellionem*. He discusses the nature of the eye, and at p. 178. he begins a set of propositions on the manner in which it acts as the organ of vision. The 28th of these propositions is that which makes most immediately for my present purpose; it is at p. 200, and he enumerates it in the following words.

“ Qui remota distinctè vident, propinqua confusè; iis per-
“ spicilla convexa profunt. Qui vero confusè vident remota,
“ juvantur concavis perspicillis.”

The mere quotation of these words is sufficient for the proof of what I have asserted; but as it may afford some amusement to your readers, I will add his remarks upon them as shortly as possible.—“ Quanta admiratio” he says, “ rei tantæ tam late propogatum usum: & tamen causum ignorari hæcenus.” J. B. Porta, he adds, professes to give the reason in his optics: but this book he was never able to meet with. Kepler then very ably refutes the opinion of those, who suppose this effect to be produced by the increasing or diminishing of the apparent magnitude of the object. He offers with considerable diffidence his own method of accounting for the phenomenon. He supposes that in the cases mentioned in the proposition the vision is imperfect, in as much as without the intervention

tervention of convex or concave lenses, the apices of the different cones of rays would fall either before or behind the retina. He supports his theory by experiments which he tried on short and long-sighted persons; and quotes the passage from Aristotle, in which he points out the difference between the myops and the presbytes.

Feb. 12.

II.

Construction of a Line in a Circle, nearly equal to the Side of a Square of the same Superficies as that of the Circle itself. With Remarks on Pendulums and other Objects. By Mr. J. WHITLEY BOSWELL.

To Mr. NICHOLSON.

SIR,

Feb. 9, 1805.

HAVING often found a difficulty in computing the pressure Introduction. of fluids in tubes and cylindrical vessels, from all the calculations relative to the gravities of different bodies, which I was acquainted with, being adapted for cubic measure, it appeared a very desirable matter to find an easy method of converting round into square measure, which induced me at different times to attempt it, and as often to lay it aside, from not being able to arrive at any satisfactory result. But lately having had occasion to resume this subject, I have discovered a method of coming so very near the truth, that if it should turn out to be in reality not so exact as it seems to me, it will serve so well for common computations, and promises on that account to be of such utility, that I am induced to send it to you for publication, if you approve of it.

As this subject has been by many looked on as somewhat To convert chimerical, it may be therefore not amiss to mention, for those round measure of your readers who are not very conversant in geometrical into square. studies, that a mechanical method of computing square measure from round is a very ancient and well-known problem; but this method being difficult to apply to the inside of tubes, and not capable of being used in diagrams or drawings, has made another method desirable: mine for this purpose is as follows:—

To

To find a right Line, the Square of which shall be equal to a given Circle.

Problem.

To find by construction the side of a square equal to a circle.

Draw two diameters (I F and C T) bisecting each other at right angles in the center (O) of the given circle; bisect one of the radii so formed (O T) in W, and from the extremity (I) of the next adjoining radius, through W draw the right line (I B) to the circumference: This line (I B) is the line required.

Proof.

Proof by trial.

Let a square be formed equal to the given circle by the third corol. prop. fifth, of Archimedes; then take a square formed by the line I B, and place it on this other square, so that one angle and the side adjacent to it of one, shall fall on one angle and the side adjacent to that angle of the other; then will it be seen that all the other angles and sides of each will coincide, and the whole of one be equal to the whole of the other.

This kind of proof is nearly the same as that of the fourth prop. first book of Euclid, on which so many other propositions depend; and having often tried this method in the above manner, I could never perceive any difference between the two squares: In those trials I used circles of card paper for the more exact measurement of the circumference in the mechanical process directed in the method of Archimedes.*

To the above I have to add the farther proof which follows of the exactness of my method, which may make it appear still more certain.

Sometime after I discovered the above, looking into a work of the learned Kircher for a ready method of describing a pa-

* In order to shew how near Mr. B.'s construction approaches to the truth, we may observe, that when the diameter is = 1, the area is = 0.7854, and the side of the equal square = 0.8862. But in the figure, I W is found by adding the square of the radius to that of the half radius, and extracting the square root; and then by the property of similar triangles, as the radius is to I W, so is the diameter to I B, which will be 0.8944 when the diameter is = 1. But this line exceeds 0.8862, or the true side of the square, by 0.0082, or nearly one hundredth part.

N.

rabola which I knew was inferted in it, I perceived therein also a geometrical method of finding a right line equal to any given portion of the circumference of a circle; and having by this method drawn a right line equal to the fourth of the circumference of a circle, and with it and the diameter formed a parallelogram equal to the circle, and then a square equal to this parallelogram, in the usual manner: I afterwards formed another square equal to the same circle, according to my method before described, and found no perceivable difference between it and the former square.

The discovery of a fact in geometry often leads to another; one of this kind I have to add here, which is, that a right line (BE) drawn from the extremity B of the line IB, at right angles through the opposite diameter (IF) to the circumference, will be equal to a fourth of the circumference.

The problem of Kircher above mentioned, may be found in the *Ars magna Lucis et Umbræ*, prop. 3, lib. 3. pars 2, page 239, and the two following pages.

As the circles which I used were under three inches diameter, it is possible on so small a scale a minute error might not be visible; but even in this case it must be very inconsiderable.

I take the opportunity of forwarding the above to send an Properties of pendulums, &c. explanation in reply to the annotations on my paper on pendulums, which I hope you will do me the favour to insert, as you have in some degree called on me for it.

I beg leave to observe, that in that paper I have endeavoured to put a marked distinction (though perhaps not sufficiently forcible) between the observations which admitted of proof, and speculations on new subjects: The latter may be considered to commence with Huygens' pendulum; and the others on similar principles, along with this, were merely designed as objects *proposed for experiment*, not as things proved, and nothing is inserted about them but what was intended to be noted as conjectural by the mode of expression.

I consider it useful to propose experiments (exclusive of my partiality to this mode of ascertaining facts), because among your numerous readers there might be some who, with little trouble or expence, have opportunities to make trials of this kind,

Properties of
pendulums, &c.

kind, from the nature of their employments furnishing them with all the materials necessary ready at hand, and who have sufficient interest in the subject to induce them to undertake them.

I acknowledge that (as you observe) I did not state all my reasons for thinking that Huygens' circular pendulum, and the others proposed on the same principles, would not be so liable to alter their notation of time, as those in common use, from a change of temperature; but this was caused by the other subjects contained in that paper having extended it to such a length as to oblige me to curtail this and other matters, and leave out part of what I had written relative to pyrometers also. My reason for thinking well of Huygens' pendulum proceeded partly from my respect for his opinion, founded on the high idea I have of his mathematical erudition; and because it appeared to me, that the size of the circle produced by the gyration of the ball, depending on the impulse of the moving power more than on the length of the suspending string; that if the string lengthened a little, the circle would not be thereby increased; and even if it was increased, that the effect of the paraboloidal lamina would prevent its altering the time of each revolution of the ball (granting the theorem of Huygens, which I inserted, to be true); for though I was aware that the parabola generated would, by the lengthening of the string, become of somewhat less curvature, yet I thought this change would be so minute as not sensibly to affect the time, especially as a change apparently greater had not affected it in a similar case, which will be mentioned a little farther on, but of this I had doubts then, as appears from what is remarked in page 77, where I mention that I thought the other constructions which I proposed might be better on account of the string of that of Huygens being liable to lengthen.

I recommended the other pendulums, on similar principles, for trial, because though the expansion will increase the dimensions of the parabolical and cycloidal curves, yet still they will not cease to be those curves, which Huygens has proved to have such remarkable properties for the regulation of pendulums; and as it has been found by experience, that a pendulum vibrating in a small arc of a large circle, has the same accuracy as when moving in a cycloid, I imagined that there could not at least be more difference between the effects of

two cycloids or parabolas a little differing in size, than between the cycloid and large circle; and besides, as the cycloidal cheeks used by Huygens (which he found to succeed so well) were liable to the same alteration by expansion, I looked on his numerous experiments as confirmations of this opinion.

Properties of
pendulums, &c.

In reply to the observation relative to the superior effect of pendulums detached as much as possible, I beg leave to observe, that a movement interrupted or intermitting (such as more or less takes place in clocks with oscillating pendulums), is so very different from one which continues equally without any interruption (as that of the circular pendulum), that it does not seem to me conclusive to argue from the effects of one to those of the other.

In the note relative to the circular vessel enclosing the mercurial tube, I am inclined to imagine you were not aware that this vessel was directed to be fastened to the spindle so as to revolve with it, by which means there would be no lateral motion given to the air but by the friction of the outside of the case; for which reason I mentioned that it should be made very smooth externally: I also directed it to be covered, to prevent the effect of the current of air, which would otherwise pass through it, caused by the centrifugal impulse of the circular motion.

As to what I observed about the suspending spring, I perhaps expressed myself too generally: I still think, however, I could point out several instances of gridiron pendulums, where the compensations were made of one bar against another only, without allowing any compensation for this spring.

I think it necessary to mention among the emendations of my former paper, that the cycloidal bed of the rolling pendulum, proposed in it, should be formed of the same thickness in every part, as it might expand unequally if made thicker in one part than another: As a trial of this sort of pendulum may be made with a common clock at a small expence, I hope it will induce some one better skilled in such experiments than I am, to make it, even if the others should not be tried: and as a farther reason for expectation of success in the trial, it should be considered (in addition to what has been advanced in support of the opinion, that the effect of the cycloidal bed would be but little altered by its expansion),
that,

Properties of
pendulums, &c.

that, on account of the shortness of this bed, it could not possibly expand as much in length as the 39 inches of wire contained in a common pendulum-rod; and that, as the greatest expansion of the wire is certainly in the direction the most injurious to the effect of the pendulum, which is not the case with that of the cycloidal bed, it is probable that the rolling pendulum would be found superior to that in common use.

With regard to the observations on pyrometers, I own I never saw that of Deluc; but as your description of it is not sufficiently minute to enable me to perceive how the standard in it for measuring the different bars, could escape expansion in an increase of temperature of the atmosphere, I still can conceive no method by which the standard for measurement in any pyrometer, could be prevented from altering its length from the above cause, but by keeping it artificially at a fixed temperature, by some means distinct from the apparatus, similar to those of Mr. Ramsden, which you have mentioned. What I wrote on this subject was intended as a caution in future experiments, and if it has produced the more extended publication of means to avoid the error I pointed out, or shall hereafter do so, my view will be fully answered.

Your very humble servant,

J. WHITLEY BOSWELL.

III.

Some Remarks upon the Experiments by which Mr. Ez. WALKER has endeavoured to explain the apparent Enlargement of the Moon near the Horizon: with a Statement of some Facts upon which that Phenomenon seems chiefly to depend. In a Letter from C. H.

To Mr. NICHOLSON.

SIR,

Mr. Walker's
experiments ad-
verted to.

I SHOULD, with your permission, be glad to point out to your correspondent, Mr. Ezekiel Walker, a theorem in optics, to which, it appears to me, that he has not paid sufficient attention in the course of his studies.

Rays

Rays from a distant object which pass through a convex lens near its center, will have their focus more distant from the lens, than those rays which pass through it nearer to its circumference; and, consequently, the image formed by the central rays, will be as much larger than that formed by the rays of the circumference, as the focal distance of the former shall exceed that of the latter. With a given aperture, the focus will be that point in which the greatest number of rays coincide, for, there, will the image appear most distinct; and, consequently, as we enlarge the aperture of the lens, the central rays must be combined with others more convergent, the new focus will approach the lens, and the image be, of course, proportionably diminished.

The total disagreement of this theorem with the result of Mr. W.'s experiments, I can only account for by supposing this gentleman, while he enlarged the aperture of his lens, not to have paid attention to the increasing brightness of the image, which, by making a stronger impression on the retina, would appear to increase in magnitude with every enlargement of the aperture. If I might be allowed to offer my advice to Mr. W. I should recommend him to repeat his experiments, and substitute, for the flame of the candle, a screen having a circular opening covered with thin paper. This, with a light behind it, may represent the horizontal moon. Some characters should be marked upon it, in order to determine, with more facility, when the image is most distinct. The precaution of reducing the different images, as nearly as possible, to the same degree of brightness previous to measurement, must not be neglected. I am surprised indeed, that the structure of the eye, which Mr. W. professes to have kept in view in the course of these experiments, did not point out to him the importance of this circumstance, which is so admirably answered, in the natural organ, by the contraction and expansion of the pupil.

As the phenomenon of the horizontal moon has of late much engaged the attention of several of your correspondents, I will avail myself of the present opportunity to add a few remarks on that subject. It appears, at first view, that the most obvious method of accounting for the phenomenon, is the following: We believe the moon more distant from us when in the horizon than when in the meridian; but we see

it,

it, in both cases, under the same apparent angle, and, according to our usual habits of judging of the magnitudes of objects, necessarily refer the idea of greater magnitude to the greater distance. The aerial perspective, and various other circumstances, which combine to make the horizontal distance appear the greatest, have been so well and so often described, that it would be superfluous to repeat them here. I must observe, however, that this appearance of greater distance in the horizontal moon, though the first and most obvious circumstance which presents itself to explain the illusion, is not, in my opinion, that which has the greatest share in producing it.

but these have not the greatest share in producing it.

After having considered the subject with much attention, I am of opinion that the chief stress ought to be laid on the following particulars :

New explanation. The moon is rarely seen in the horizon.

Small objects eclipse the moon at considerable altitudes.

The elevated clouds are also large, because near ; and the moon seems small, because compared with them.

But in or near the horizon, the clouds, buildings, and other large objects, will appear small, and hide but little of the moon's disc ; which will therefore, by comparison, be seen on a grand and enlarged scale.

These are the principal circumstances,

We may be said rarely to see the moon in the horizon. Our habitual acquaintance with it is at some considerable altitude. Hence it follows, that all the objects with which it comes in apparent contact, and with which we occasionally compare its disk, are near to us, and subtend angles proportionably large. Thus, for instance, we often see the moon lose a large portion of her face behind the branch of a tree or a weather-cock, and totally disappear behind a chimney. The clouds, too, which fleet before her, appear, from their proximity to us, on a gigantic scale. These are the circumstances under which we have insensibly formed our general ideas of the moon's magnitude. Let us now survey it in the horizon. The case is widely altered. We have now an opportunity of comparing it with various large objects, which perhaps, being themselves on the verge of a distant horizon, will be reduced to very trifling dimensions ; and thus the castle and massy cathedral will hide but a small part of that globe, which we have commonly seen half eclipsed by a spout or a weathercock. The clouds too, reduced by distance, appear on a much smaller scale when compared with the moon. In short, it is no longer the same moon we were acquainted with in the meridian, but a much larger and more majestic sphere, the novel appearance of which strikes most of its spectators with a degree of awe and amazement.

Though it appears to me that the circumstance I have pointed out, of our habitual comparison of the moon with objects

objects of known magnitudes, has the greatest share in producing the illusion by which it appears largest in the horizon, I would by no means be understood to reject the principal circumstances, long since noticed as contributing towards the same effect. Different circumstances will necessarily have more or less influence with different persons, according to the tenor of their previously acquired habits and observations.

I remain, Sir,

Your obedient servant,

C. H.

Tavistock Place, Feb. 15.

ANNOTATION. W. N.

THIS very striking and perspicuous explanation of the enlargement of the moon near the horizon, will bring to the reader's recollection a great number of facts, where the absolute magnitudes of objects, of which the distances are imperfectly known, are erroneously estimated by comparison with other objects lying in the same direction. Navigators must have been often struck with the extreme minuteness of the image of the sun or moon, when brought down by Hadley's quadrant so as to be seen projected upon the near objects below the horizon. Since the perusal of this communication, I have repeated the experiment on those luminaries. If the image of the moon be removed a certain number of degrees from the direct ray, it is easy, by altering the position of the quadrant, to observe that image in the horizon to the right or left, or upon the pavement before us, or at a great elevation in the heavens. In these cases its apparent magnitude, while the angular size and brightness remain unaltered, is found to be small at the great elevation, large in the horizon, and most so when projected upon distant objects, and it becomes a minute speck when seen depressed among the objects close to the observer. The same experiment may be made with a pane of glass, but less objectionably, because it might be urged that the quantity of illumination is different according to the obliquity of reflection.

Experiment with Hadley's quadrant, in which the sun and moon appear of different magnitudes, according to the objects on which their images are projected.

IV.

A Mathematical Theory of the Speaking Trumpet. By JOHN GOUGH, Esq.

To Mr. NICHOLSON.

SIR,

Theory of the
speaking trumpet.

THE speaking trumpet is a conical tube, which receives the human voice from the mouth, and encreases the range of it. The structure of these instruments, and the materials of which they commonly consist, teach us to refer this singular faculty to two causes. In the first place, the æriel pulses undergo certain modifications in the cavity of the trumpet; the nature and effects of which I am going to investigate. Secondly, the metallic shell of the instrument augments the power of the voice, by its aptitude to conduct those impulses; which are impressed upon it, partly by the pulses of the included air, and partly by the direct action of the Larynx, transmitted through the medium of the face to the mouth-piece of the instrument. Before I enter upon the investigation of the effects resulting from the first cause, it will be necessary to premise the following lemma:

Article 1. Let $OR Sr$ (Fig. IV. Plate 7) be an evanescent sphere of air, which is agitated by two or more pulses, beginning and ending together, and radiating from the points Pp , &c. the vibratory motion of $OR Sr$, in any direction whatever is equal to the joint sum of the forces, which the pulses would impress upon it separately. Join PO , Op ; and draw the lines ROr , SOs perpendicular to PO , Op ; now as these lines are evanescent, every point of each of them is equally pressed at any instant by its respective pulse. But the force imparted to ROr by P , agitates the sphere ORS equally in all directions, *Principia*, 422; the same holds true of the force of p upon SOs . Now these forces cannot coalesce or be compounded so as to act in a single direction,—*Manch. Mem.* V. 5. p. 660; consequently they act with their joint powers in all directions.

Art. 2. This being premised, let the frustum $ABCD$ (Fig. 3.) represent a trumpet, the shell of which is a non-conductor of sound. After a pulse of the voice has passed through the aperture

aperture A B, it is bounded in front by a spherical surface, having O, the vertex of the cone D O C, for its centre, and subtending the plane angle A O B; in other words, let such a surface be described any where in the frustum A B C D, and the variable density of any one particle of air in contact with it, will be equal to the cotemporary density of any other particle in the same surface. For as soon as the motion is propagated into the internal air, the pulse will expand as far as the sides of the tube will permit: *Princip.* 42.2: after which, the motion will be continued to the opposite aperture C D, in right angles, 43 *ibid.* therefore it will be propagated along the lines A D, B C with equal celerities, suppose to T and V.— On the plane T V describe the surface T N V, in which the density of the air is every where equal at any instant: Now every point of such a surface acts upon the air externally in contact with it, and is re-acted upon by the same perpendicularly to itself, in such a manner that equal portions of the surface support equal degrees of pressure. Let U u be an evanescent particle of the curve T N V, draw U K, u k perpendicular to N O; then the superficial ring between the planes U K, u k is as U K \times U u; but this ring is as the pressure upon it; which is also inversely as the radius of curvature at U, therefore if U K \times U u be constant, the radius of curvature is constant; consequently it is equal to O T, and the truth of the proposition is manifest.

Art. 3. Let f , be the force of a pulse in the aperture A B; and let the internal air of the cone be agitated with an equal and similar impulse at the point O; then the effect thus produced in the trumpet, will be the same with that of the voice acting at A B, (by *Art. 2.*) But had the cone been out of the way, the same force at the distance O A, would have been uniformly diffused over the surface of the hemisphere, having O for its centre.

Art. 4. Put A O = r ; the variable distance O T = x ; the radius of an evanescent circle = d ; the versed sine of half the angle A O B = p ; 3.1416 = c . Then as the force f is diffused over the hemisphere whose radius = r , in free air by *Art. 3.* that part of it which resides in an evanescent point of the same surface = $\frac{f d^2}{2 r^3}$.

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speaking trumpet.

Art. 5. The magnitude of the surface $T N V = 2 c p x^2$: but the force f is diffused over this segment by *Art. 3*, consequently that part of it which is confined to an evanescent

$$\text{space } q = \frac{f q}{2 c p x^2}$$

Art. 6. Since the spherical surface $T N V$ is convex to the plane $C N D$ (*Art. 2.*) the agitation of the air in the latter will commence at the centre N , and extend thence by the application of successive circles of the sphere $T N V$ to equal circles of the plane $C D$, having N for their centre; which operation will continue until T and V fall upon C and D .—Now as $C N D$ is in contact with unconfined air, each point of it, upon being struck, will become a centre, from which a pulse will radiate freely. (*Princip. 42.2.*)

Art. 7. Let P be such a physical point, and let its area $= q$; then if an evanescent plane $= q$ be drawn through P perpendicular to $O P$ or x , the force impressed upon it by the trumpet will be $\frac{f q}{2 c p x^2}$ (*Art. 5*) but the same force is imparted at the same time to the equal physical point P in the plane (*Art. 1.*)

Art. 8. Let L be the place of an ear, in $O N$ produced, or more properly of a minute sphere of air. Put $N P = y$; $P L = w$. Now the pulse proceeding from P will have due effect upon the point L (*Manch. Mem. v. V. p. 662. cor. 1*); which effect is as $\frac{f q}{2 c p x^2 w^2}$ (*Arts. 6 and 7*): hence we have (by *Art. 4*) as $\frac{f}{r^2} : \frac{f d^2}{2 r^2} :: \frac{f q}{2 c p x^2 w^2} : \frac{f q d^2}{4 c p x^2 w^2}$ for the force acting upon the sphere L having d for its radius. But the number of points which act together $= \frac{2 c y y'}{q}$ (*Art. 6.*); consequently their united forces $= \frac{2 f d^2 y y'}{4 p x^2 w^2}$ (*Art. 1.*)

Art. 9. Put $L N = g$; $N O = e$; $O C = a$; $C L = b$; and the correct fluent of the preceding expression is $\frac{f d^2}{4 \times p g^2 - p e^2}$ drawn into the hyp. log. of $\frac{g a}{e b}$; when g and e are unequal. But if $C D$ bisect $O L$; put $C N = k$, and the correct fluent becomes $\frac{f d^2 k^2}{4 p g^2 b^2}$.

Art. 10.

Art. 10. Though the foregoing fluents have received one ^{Theory of the speaking trumpet.} correction, each of them requires a second, arising from certain circumstances peculiar to this problem. For it is evident, that the commencement of the agitation at N is prior to that at C, by a part of time, which is as $CO - ON$; consequently the completion of the former pulse at L, will precede the termination of the latter at the same point by a similar interval; which is as $LC + CO - OL$. Put this time $= t$, and the duration of one vibration of the larynx $= l$; also let the indivisible interval, or $\frac{1}{36}$ of a second according to Euler be denoted by m ; then multiply the expressions in *Art. 9* by the fraction $\frac{m-t}{l}$.

Art. 11. The force determined in the preceding paragraphs, is that which a non-conducting tube imparts to an evanescent sphere of air. This circumstance affords a plausible objection to the present theory; because the sense of hearing is not confined to a physical point! But as the seat of this sensation is of a given extent, at least in each person, the number of these vibrating points, in contact with the sensitive surface, is also given: from this it follows, that the effect of one particle is nearly a true measure of the power of the whole number, when the ear lies at a great distance from the plane CND, representing the trumpet's mouth.

Art. 12. So much of the present sheet is occupied by the consideration of the first cause, that my remarks on the second, or the effects of the metallic shell, must be confined within narrow limits; these I shall begin as follows.

Art. 13. Since the conducting power of the trumpet is given, the vibrations communicated to the small end of it by the breath and face, will pass to the opposite extremity in a given time; which call L. Also let h be the force of a vibration at the distance 1; and CL or b express the space betwixt the trumpet and a remote ear.

Art. 14. In the first place, let l be equal to L; then a single pulse of the trumpet will strike the ear for every vibration of the voice; and the force of it will be $\frac{h m d^2}{L b^2}$ by my former paper on this subject. In the next place, let l be an aliquot part of L; then the number of pulses impressed on the ear in

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pet.

the time of L is $\frac{L}{l}$; hence the force $\frac{h m d^2}{l b^2}$ Add this

equation to the former of those found in *Art. 9*; and the sum gives the force of the trumpet upon a distant ear.

Art. 15. But if l be an aliquot part of L , the sound will be imperfectly formed; because the quantity $\frac{L}{l}$ being a mixed

number, the vibrations of the larynx will be conducted along the metal, and discharged into the air in a disorderly manner, so as to destroy the continuity of the note. The same cause, in all probability, constitutes the difficulty which the inexperienced find in their first attempts to sound horns and military trumpets: for the lips, which are compressed by the end of the tube, form the mouth piece in this case; and the art of producing the sound seems to consist in causing the edges of them to vibrate in aliquot parts of L , or the interval belonging to the lowest note of the instrument. The same origin may also be ascribed to the musical scale, composed of what are called the trumpet notes; the intervals of which decrease in the following ratios, $1, \frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \frac{1}{5},$ &c. for $\frac{1}{2}$ is the greatest aliquot part of unity, $\frac{1}{3}$ the next in succession, and so on.

Art. 16. If the remarks contained in the two preceding paragraphs be just, the person who makes use of a speaking trumpet, must attend to the management of his voice; the pitch of which ought to coincide with some note in the trumpet scale of his instrument: for proper attention to this circumstance will make the sound of the metallic shell perfect, and bring it into unison with the other sound; which arises from the ærial pulses, passing through the cavity of the tube, at the same instant. In this manner, the greatest sound will be produced, supposing the power of the voice to be constant; because the two sets of pulses, constituting the unison, will impress the greatest possible force upon the ear in a given time. On the other hand, if the foregoing precaution be neglected, the imperfect sound of the metal will disturb the uniformity of the other, which is formed in the cavity; and the force impressed on the ear by so disorderly an aggregate of pulses, will be interrupted and inconstant.

Art. 17. Perhaps it is unnecessary to remark, that the perfect elasticity of the air forms the basis of the present theory; a supposition

supposition which in all probability is not altogether just; but, when the smallness of the angle O L C is recollected, the error arising from the hypothesis vanishes in a great measure.

JOHN GOUGH.

Middleshaw, Feb. 8, 1805.

V.

Observations on Basalt, and on the Transition from the vitreous to the stony Texture, which occurs in the gradual Refrigeration of melted Basalt; with some geological Remarks. In a Letter from GREGORY WATT, Esq. to the Right Hon. CHARLES GREVILLE, V. P. R. S. From the Philosophical Transactions for 1804, p. 279.

(Concluded from Page 126.)

A CURIOUS diversity may prevail in the products of a Fusion and compound body subjected to fusion, when absolute solution is produced. When merely simple fusion takes place, the aggregation of the parts only is destroyed: the fluidity arises from the facility with which they move on each other; and a regulated diminution of temperature, by facilitating their re-union, can hardly fail to recompose the same species that formerly appeared to exist in the compound. But, if the molecules have been dissolved and decomposed, and their component particles diffused through the fluid, there seems to be very little probability that any re-union should compose the same molecules. It is more likely that new compounds will be formed, from which new molecules, and of course new crystals, will be generated; and that, consequently, the same rock may become the parent of very diversified offspring. These will however retain some traces of their origin; for, as there can be no fusion of a compound body imagined, in which the mutual action of the components will not decompose some portion, there can be no solution supposed so perfect that every molecule shall be destroyed. In the first case, there will exist the germs of a new composition; and, in the second, there will remain the relics of the old.

If these observations are correct, considerable utility seems derivable from them, in the explanation of some geological problems. The aqueous formation exhibits similar facts.

problems. It will appear, that they strikingly illustrate the analogy which exists between the aqueous and igneous formations; and show that precisely the same order and kind of arrangement is followed, in the generation of stony masses from water as from fire; for, the change of structure, which I have observed to be the most inexplicable part of the process by which glass passes into stone, is almost exactly imitated in the formation of calcareous stalactites. Successive depositions of calcareous carbonate, form a stalactite, which at first is fibrous. A continuance of the process causes the fibrous structure to disappear, and the stalactite becomes irregularly spathole. The irregularities then vanish, and it becomes perfect calcareous spar, divisible into large rhomboids, with the form peculiar to that mineral; and all the gradations may be found in the same specimen. Nor is this change confined to a few solitary specimens; for a considerable extent of coast near Sunderland, is formed of a lime-stone composed of radiated spheroids, from half an inch to three inches diameter, imperfectly united. When one of these spheroids attains something more than the usual magnitude, it becomes compact in the heart; and it is not unusual to discover portions of the rock, in which the radii have entirely disappeared, and the whole mass has become compact. It is probable that the entire formation of oolithi and pisolithi is owing to the same cause; and that they are prevented from ever arriving at great size, by the union of their surfaces, and their subsequent consolidation into compact limestone, into which they are continually found to graduate.

Calcareous stalactites: at first fibrous, then irregularly spathole, then spar.

Coast near Sunderland to form-
ed.

Petrofalex, of doubtful origin, similarly arranged in its structure.

Hitherto, I have selected instances from substances which have an undisputed claim to an aqueous origin. I shall now, on the authority of Dolomieu, instance a similar arrangement, in a substance respecting the origin of which theorists are not agreed. A species of petrofalex is found in the Val de Nido, in Corsica, which contains radiated petrofaleous glands, from half a line to an inch in diameter. These glands only differ from the basis by their radiated structure, and their colour; and appear to indicate very clearly, that the rock was subjected to a species of arrangement which, if it had been completed, would have changed its nature, and probably would have rendered it porphyritic; for Dolomieu observes, that the centre of the glands was often occupied by a small crystal of feldspar.* The extra-

* Dolomieu. Journal de Physique, 1794, page 260.

ordinary rock called the globular Granite of Corsica, is an analogous instance. It is composed of crystals of hornblende, feldspar, quartz, and mica, in confused aggregation; and in this basis are immersed spheroids, about an inch and a half or two inches in diameter, composed of concentric alternate coats of quartz and hornblende. The centre is principally occupied by hornblende; this is surrounded by a zone of quartz. These spheroids are radiated to the centre. There can be little doubt that this rock is merely the result of interrupted crystallization; and that, if the process of arrangement had continued, this structure would have disappeared, and the whole rock would have resembled the present basis. Hitherto, this very singular rock has only been found in detached fragments.*

The admission that solution is not a requisite of crystallization, appears to me an important concession in favour of the aqueous system, which has laboured under very great embarrassment, from the difficulty of dissolving quartz. If a very perfect mechanical suspension be all that is requisite, we may

Solution not being indispensable to crystallization, but only mechanical suspension, many geological difficulties are removed.

* I shall venture to quote another instance, on the authority of Professor Playfair. "The salt rock in Cheshire, which lies in thick beds, interposed between strata of an argillaceous or marly stone, and is itself mixed with a considerable portion of the same earth, exhibits a very great peculiarity in its structure. Though it forms a mass extremely compact, the salt is found to be arranged in round masses, of five or six feet in diameter, not truly spherical, but each compressed by those that surround it, so as to have the shape of an irregular polyhedron; these are formed of concentric coats, distinguishable from each other by their colour, that is, probably, by the greater or less quantity of earth which they contain; so that the roof of the mine, as it exhibits a horizontal section of them, is divided into polygonal figures, each with a multitude of polygons without it, having altogether no inconspicuous resemblance to a Mosaic pavement. In the triangular spaces without the polygons, the salt is in coats, parallel to the sides of the polygons." Illustration of the Huttonian Theory, page 37.

I am informed, that the siliceous deposition at Geyser, is at first a porous friable mass, and that the addition of more molecules renders it fibrous; also that, on a farther addition, the fibrous structure disappears, and the whole assumes the compact even texture of chalcedony or flint. If I am not misinformed, a series of specimens illustrating this transition, existed in the cabinet of the late Dr. Hutton, of Edinburgh.

cease to wonder at the almost daily formation of petrified wood, (in which, though crystallization does not actually take place, a very perfect arrangement is indicated, by the intimate union of the siliceous particles,) or of hydrophanous semi-opals in the decomposed serpentine of Mussinet, near Turin, or of chalcedony containing drops of water, in the decomposed basalt of Vicenza.

Since the crystallizing may not take place in the order of fusibility, refractory substances may be impressed by such as are more fusible.

I have endeavoured to show, that in the crystallizations resulting from igneous fusion, it is not only possible but probable, that the most infusible substances might not be the first to crystallize; and this appears to involve important consequences, for it partly removes one of the greatest difficulties that embarrasses the igneous theory, by explaining the possibility of refractory substances generated by fire being impressed by the forms of more fusible ones. It seems, however, that the same order of arrangement would prevail in substances that were suspended in a fluid medium, as the degrees of attraction would be the same. In either case, the first step by which the arrangement of an apparently homogenous mass commenced, would probably be the accumulation of particular molecules into little globules. Such seems to have happened in variolites, and other rocks which contain spherical concretions of a different nature from their basis. Still farther advanced is the arrangement of porphyries: the molecules of one species have assumed a regular crystalline form; and sometimes two or even more varieties of crystals are formed, which remain unmixed in the unarranged basis. If the remaining molecules of that basis are susceptible of crystallization, it may be fairly concluded, that an extension of the process of arrangement would convert the porphyry into granite, or at least into one of the compound aggregates of crystals which constitute the numerous tribes of granites, grunsteins, and sienites; and it seems equally probable that this might be accomplished, whether the molecules were indebted to a suitable temperature, or to an aqueous medium, for the requisite facility of movement.

Instances and remarks.

Granites and porphyry.

The formation of granite and other rocks, must however be referred to the ultimate perfection of crystallization, by which all the molecules have been permitted to arrange. Those granites called porphyritic, in which large crystals of feldspar are imbedded in a basis compounded of the ordinary ingredients of granite in small grains, are apparently generated from a menstruum

strum in which the molecules of one species, being greatly predominant in number to the rest, are the first to exercise their polarity, and constitute large crystals, which are afterwards surrounded by smaller ones, resulting from the successive separations of the remaining elementary molecules.

The changes of the substance that led to the foregoing remarks, serve to show that they are not altogether hypothetical; and any proof that may appear deficient, seems to be provided by the phenomena exhibited by lavas, in which may be observed every step of the passage from the vitreous to the stony, from that to the porphyritic, and finally to the granitic state. The lava of Lipari, which passes from glass to lava, by the generation of minute globules, may be cited, on the authority of Spallanzani, as an instance of the commencement of the process of arrangement; * and, were not their origin still disputed, might also cite the pitchstone lavas of the Euganean hills. It would appear, that the transition from the stony to the porphyritic state is rapid, for perfectly homogeneous lavas are among the rarest of volcanic products. The porphyritic lavas are most numerous; and it is needless to detail the varieties they present.

The passage from the vitreous to the stony state, from that to the porphyritic, and lastly to the granitic state are observable in lavas.

* Spallanzani, *Viaggi alle due Sicilie. Tomo Secondo*, page 239. The whole passage, literally translated, stands thus: "This lava has a basis of feldspar, of a fine and compact grain, a splintery fracture, rough to the touch, and emitting sparks, like flint, when struck with steel. It has an ash colour, in some places approaching to a leaden colour. It is thickly filled with an immense quantity of little bodies, which would be distinguished with difficulty, from the resemblance of their colour to that of their basis, were it not for their globular form. But this lava is joined to a great mass of glass, which forms a whole with it, without any division or separation between them; and this lava, which in many places retains its own nature, is in many other places reduced to glass. Some parts of this glass are filled with the same little bodies, but other parts are pure glass. This is in general very compact, has a dead black colour, and breaks rather into irregular pieces than into undulated fragments, as glass properly does. Besides, it has I know not what of unctuousity to the touch, and to the eye, which is not perceptible to the more perfect volcanic glasses. It yields sparks with steel, like the lava; but the lava is wholly opaque, and the glass, at the angles and thin edges, has considerable transparency. It is only opaque where the globules are, which appear to be particles of lava."

But,

But, though the process of arrangement has often only advanced thus far, it has in many instances proceeded much farther, and it is by no means unusual to find the entire basis regularly arranged into crystalline bodies; thus, to cite a well known instance, in many of the ancient lavas of Somma, large augites are imbedded in a crystalline mass, formed of minute crystals of leucite, together with another crystalline substance, whose nature is not perfectly determined.

Volcanic glass.

The casual occurrence of volcanic glass is nowise at variance with this account, as it is sufficiently probable, that some glasses may have a much greater tendency to crystalline arrangement than others possess; and it cannot appear extraordinary, that regular crystals should sometimes be generated, even in the glass, as it is a matter of daily occurrence in artificial glasses, and in furnace slags.

Peculiar bodies may be afforded by this method of separation.

If the distinction attempted to be shown between igneous fusion and solution be established, it may offer a means of accounting for the abundance of peculiar bodies in lava, which do not exist in other situations, or at least are of extremely rare occurrence. For, if the igneous action decomposes the molecules of the substances on which it operates, there seems every probability that new compounds may result, dissimilar to any substances we are acquainted with. It would appear, that the necessity of imagining an undiscovered stratum abounding in leucites, chrysolites, and augites, may be dispensed with; and, as I have endeavoured to show the probability that the most infusible substances will not be the first to crystallize, the penetration of refractory leucites by fusible augites, will cease to be an argument against both being generated in the lava. I may also observe, that the same causes which vary the crystallized bodies resulting from igneous solution, must operate upon the unarranged basis; and that the same rock may be fused into lavas extremely dissimilar, as their varieties must depend on the degree of solution which the fusion has accomplished.*

The facts and doctrines here given will not affect the question of the igneous or aqueous formation of basalt.

If the analogy attempted to be shown between the aqueous and igneous formation appear founded, the transition from glass to

* The evidence of the generation of leucites in the lava which contains them, collected by Leopold de Buch, and Bréislac, and finally acquiesced in by Dolomieu, appears so satisfactory, that it can hardly be deemed presumptuous to assume the point as determined.

to stone can no way affect the great question which has so long divided geologists, about the origin of basalt; for though it is synthetically demonstrated that basalt may be formed by fire, the converse of that proposition stands supported by strong analogical arguments, and its formation by water must be allowed to be at least equally possible. How far the probabilities derived from the examination of basaltic formations may influence the ultimate decision, is an enquiry in which I shall not now engage; though I cannot avoid recalling to my mind, the numerous instances of petrifications found in basalt, and, as a counterpoise to that observation, the equally numerous instances in which the heat emanating from it appears to have indurated strata, and coaked beds of coal. One remark may be stated here with propriety, as it arises immediately from the experiment which has occasioned these observations. In the ultimate result of that experiment, the arrangement of the molecules was much more perfect than in the original rock. It might be supposed, that a longer continuance of the suitable temperature was afforded it. This, however, could not be, for the mass was only a few feet long, and a few inches thick; the fire was only maintained a day; and the whole was cooled in a week. But the hill of solid basalt, from which the substance operated upon was taken, is several miles long, and several hundred feet high; and, supposing it to have been irrupted in a state of igneous fusion, it must have required months, nay years, for its refrigeration. How then comes it, that the process of crystallization is so little advanced? How comes the confusion of its texture to indicate the very reverse of the tranquillity and perfection of arrangement, which may be fairly assumed as necessarily attending the extremely gradual changes of so immense a mass?

Why so little of crystallization appears in the structure of basalt.

This objection admits of being obviated, upon the supposition that, in the process of melting, the molecules of the basalt were decomposed; and that the new ones generated were more disposed to crystallize than those whose place they supplied. This explanation is in some degree justified, by the

Probably because new molecules were formed more disposed to crystallize.

mined. Neither de Buch nor Dolomieu have been able to convince themselves that the augites were also formed in the lava; but I confess myself entirely unable to appreciate the cogency of their arguments, which seem annihilated by the admission they have made in favour of leucites.

total

total disappearance of the minute feldspar and hornblende of the basalt; instead of which, the regenerated stone contains thin laminæ of crystals, which are probably augites.

The division of basalt into globular masses during its decomposition, indicates the formation of radiated spheroids therein;

I cannot leave this subject without noticing some particulars, in which the process of arrangement described in the early part of this letter, appears to yield a probable explanation of some of the peculiarities of basalt. The general disposition of basalt to divide into globular masses, in decomposing, is too remarkable a fact to have escaped the attention of naturalists; though, as far as I am informed, no satisfactory explication of it has been given. The common effects of decomposition are obviously inadequate; for it is common to see a large block of amorphous basalt separate into numerous balls, after a few months or years exposure to the weather; and, rapid as the process of decomposition has been in the intervening portions, these balls resist its farther progress with uncommon obstinacy. May not this be attributed to the formation of the radiated spheroids, whose occurrence in my experiment I have already mentioned? and may not their greater resistance of weather simply arise from their aggregation being more perfect than that of the incoherent molecules which have filled the intervals between them? Though the radiated structure has disappeared to the eye, these portions of the stone retain the superiority of more perfect internal arrangement; and, if my pigmy experiments could yield spheroids of two inches diameter, there can be no difficulty in supposing that the grand operations of nature may produce them of several feet. The separation of the decomposed fragments in concentric coats, seems easily explained; for I have already pointed out the facility with which the radii of the spheroids separated at nearly the same distances from their centres, and the form of the fragments which resulted, resembling fragments of bombs.*

and these may have naturally formed the basaltic columns.

If this idea be not considered as entirely divested of plausibility, I may venture to extend the same principle, to account for the wonderful regularity of the prismatic configuration of

* Even granite has been frequently observed to affect globular decomposition, and division into fragments of concentric coats. This mode of decomposition extends to many substances, that Werner has called the formation it seems to indicate, "*abgesonderte Stücke*," which has been rendered in English, *distinct concretions*.

basaltic

basaltic columns, and also for their articulations. If we suppose that a mass of fluid basalt has filled a valley to an indefinite depth and extent, the process of arrangement in its particles must be induced by the removal of its heat or moisture, according as its solution is igneous or aqueous. This can only be done by the action of the atmosphere on its upper surface, and by the ground on which it reposes absorbing the heat or moisture from its under surface. From the variations of the atmosphere, its action must be irregular; and, from the perpetual change of the parts in contact with the heated or moist surface, its operations will always be nearly as active as at first, allowance being made for its variations. But the absorption of the ground will be regular, and regularly diminishing in activity, in proportion as the parts near the mass approach nearer to the same temperature, or same moisture, with the mass above; and its absorptions can only be carried on by its transmission of heat or moisture to the solid rocks below. From these considerations it seems evident, that the arrangement of the part of the basalt near the ground, will be begun with more energy than it can be continued, and that the results will be more slow and regular than the arrangement induced by the perpetual though variable action of the atmosphere. After the first stage in the process of arrangement has been performed, and a stratum, if I may so term it, of the jasperous substance extended over the surface of the ground, there seems no reason to doubt that a number of radiated spheroids would be generated in it, which would probably have all their centres about the same distance from the ground; and, as the arranging power undergoes a gradual diminution of energy, it is not probable that two rows in height of them should be formed at once, as that would indicate a hasty process, which had prepared a greater mass of matter for their almost simultaneous formation. From these considerations, there seems no improbability in supposing, that in the arrangement of a mass of fluid basalt, a single layer of radiated spheroids would be formed, reposing on the ground which supported the mass.

Enumeration of facts and circumstances.

1. Radiated spheroids formed near the ground,

in one layer.

I have already stated, that when the radii of two spheroids came into contact, no penetration ensued, but the two bodies became mutually compressed, and separated by a plane, well defined, and invested with a rusty colour. I also stated, that when several spheroids encountered, they formed one another

2. These would take hexagonal peripheries at the places of side contact;

and other spheroids formed above them would produce articulated columns.

into prisms with well defined angles. In a stratum composed of an indefinite number in superficial extent, but only one in height, of impenetrable spheroids, with nearly equidistant centres, if their peripheries should come in contact on the same plane, it seems obvious that their mutual action would form them into hexagons; and, if these were resisted below, and there was no opposing cause above them, it seems equally clear, that they would extend their dimensions upwards, and thus form hexagonal prisms, whose length might be indefinitely greater than their diameters. The farther the extremities of the radii were removed from the centre, the nearer would be their approach to parallelism; and the structure would be finally propagated by nearly parallel fibres, still keeping within the limits of the hexagonal prism with which their incipient formation commenced; and the prisms might thus shoot to an indefinite length into the undisturbed central mass of the fluid, till their structure was deranged by the superior influence of a counteracting cause, which would be provided by the action of the atmosphere on the upper surface of the basalt. If this arrangement existed, the same cause that determined the concentric fractures of the fibres of the spheroids, would produce convex articulations in the lower joints of the prisms; and, in proportion as the centre from which they were generated became more remote, the articulations would approximate to planes. If the generating centres were not equidistant, the forms of the pillars would be irregular; and the irregularity would be in proportion to the diversity of distance between the centres. If the difference was great, the number of sides would be altered, and they might be found pentagonal, tetrahedral, and trihedral. As the compression of the fibres would be greatest in the level of the generating centres, the lower part of the prisms would be most compact.

The facts attending the production of basaltic columns agree with the foregoing.

All these conditions seem to be fulfilled, in the actual formation of basaltic columns; for, in every instance I am acquainted with, they appear to have been formed in the tranquil bosom of the mass, as they have been originally masked by amorphous trap, and their prismatic structure is only displayed by the removal of this covering. This has been variously effected, sometimes by the apparent disrapture of rocks, sometimes by the exterior portions of the mass being thrown

thrown down by the failure of the ground on which it stood, sometimes by the violence of the waves, and not unfrequently by the working of quarries. In most instances, these operations have only removed the covering from one side of the colonnade; and it remains crowned, and generally surrounded, by an immense amorphous mass. Where there are two ranges of columns, with an intervening amorphous stratum, it is probable that the upper is the result of a second inundation of fluid basalt. It is well known that basaltic columns are most solid at the bottom; and their convex articulations have been repeatedly observed. Since these considerations occurred to me, I have had no opportunity of examining, whether the divisions approach nearer to the plane surfaces as they recede from the centre from which the prism was generated, nor whether below that centre the convex surface of the articulations is inverted; but I think it by no means improbable, that subsequent observations may establish this to be the case, and thus confer on this hypothesis nearly all the demonstration of which it is susceptible. I may however add, that the phenomena Basaltic veins. of prismatic division in basaltic veins, perfectly coincide with what might be inferred from the data upon which my reasoning has proceeded. In veins it is obvious that the refrigerating or absorbing cause must operate with nearly equal force on each side of the vein; and it follows, that two sets of prisms would be generated, which would be horizontal instead of perpendicular, and that, unless a mass of amorphous basalt was interposed between them, they must form a division in the middle of the vein, as, from the mutual impenetrability of their fibres, they could not incorporate. The coincidence of the existing phenomena with these conclusions, is sufficiently remarkable; for, in numerous observations I have made on the basaltic veins which effect the prismatic configuration, I found the prisms were always horizontal, and often, that there were two ranges of them. One of their ends applied to the wall of the veins, the other frequently united to an amorphous mass which separated them; and, when no such intermedium occurred, there was invariably a division in the middle of the vein. Not unfrequently, the veins contain three sets of prisms; a range of small ones on each side, and of much larger ones in the middle. In this case, the little prisms
are

are always separated from the large ones, and the divisions of the large ones are very irregular. *

These deductions do not suppose either the aqueous or igneous formation of basaltes to be established.

After the statement of my opinion, that perfect similarity of structure may exist in the products of aqueous and igneous formation, it will hardly be necessary to conclude these observations with remarking, that I should not consider the establishment of these peculiar modes of arrangement as the slightest demonstration of the igneous origin of basalt. It appears to me, that the truth of my deductions is entirely independent of either theory, and that, if ever the period should arrive when the origin of basalt shall be determined by irrefragable demonstration, the inferences I have drawn may be accommodated with equal facility to either mode of agency. †

The

* The observations alluded to were made during the course of last summer (1803), on the very numerous basalt veins, or, as they are there called, Whin Dykes, which traverse the red sandstone and red sandstone breccia, which forms the greatest part of the coast of the Firth of Clyde, between Greenock and the Largs.

† Mr. Keir, in his paper on the crystallizations formed in glass, suggests the probability of basaltic pits being formed by the crystallization of vitreous lavas. See Philosophical Transactions for 1776, Vol. LXVI. page 530.

Dolomieu was of opinion, that the prismatic form was peculiar to lavas which had flowed into the sea; and he attributed it to the shrinking of the mass: his description of the appearances exhibited by what he calls the prismatic lavas at the foot of Etna, merits quotation.

“ In the lavas of Etna, the form and dimensions of the columns vary as much as the manner in which they are grouped; hexaedral and pentaedral prisms are most abundant; then the tetraedral, the triedral, heptaedral, and octaedral. The least I have seen are only four inches diameter; others are more than three feet; they are commonly of a single shoot, which is sometimes 60 feet high; others are divided by articulations, which are from one to six feet asunder.

“ I have more than once observed a large column divide into several smaller in its upper part. The columns are generally larger near the top than the bottom of the stream of lava, because they subdivide; and they are always least in that part of the stream of lava which first entered the water, the refrigeration being more prompt,

The immense magnitude of some basaltic columns; the extreme regularity of their prismatic configuration, and the peculiar structure of their articulations, have directed the attention of naturalists to them, much more than to any of the other rocks which affect the columnar form. Yet many of these are sufficiently remarkable to deserve more particular notice than has generally been paid to them; and they afford most illustrative proof, that this configuration is not confined to either the aqueous or igneous formation; for some lavas, universally allowed to be such, are prismatic.* Columns of porphyry

Other rocks also affect the columnar form.

prompt, and its effects more marked. Sometimes the columns are placed perpendicularly side by side, and form vertical walls, which are sometimes more than 100 feet high, and a league long; sometimes they are heaped obliquely, horizontally, and in all positions. Some, without being divided in their length, are larger at one end than the other; and then they are arranged like wood piled up, with all the small ends at one side; sometimes they are formed into pyramidal bundles, by parting from a common center; and, finally, there are some which, by their reunion, form large balls. These radii of lava, which are rather pyramidal than prismatic, resemble those of the globular pyrites, striated from the center to the circumference, which are found in the chalk of Champagne.

"On the shore of la Trezza, near the mole, there is a very curious group of little articulated prisms, which issue from a common center, and form fasciculi singularly twisted. The articulations are marked, but the species of vertebræ do not separate. In the heart of the mountain on which stands the Castello di Jaci, there are large balls, from two to four feet in diameter, resembling in form the large pyrites in the chalk of Champagne. These balls of lava are formed of pyramidal columns, united by their points in a common center." *Catalogue des Laves de l'Etna*, page 453.

The division of the upper part of basaltic columns into several smaller ones, has also been observed in the basaltic columns of Fairhead, by Dr. Richardson. See Nicholson's Journal, 4to. Vol. V. page 321.

* Almost all the prisms at the foot of Etna, described by Dolomieu, are of dubious origin; most of them are probably basalt. The columns of the Vincentine are of the same substance, and so are the prismatic lavas of Auvergne, and of the Vivarais. The bed of lava at la Scala, near Portici, is divided by vertical fissures, which give it the aspect of irregular columns. At Aquapendente, in a quarry of undoubted lava, near the road, are some much more

Instances: Porphyry are not rare; and, among other places, are found near Dresden, several feet in length, and not more than two inches in diameter. Columns of petroflex compose a large portion of a mountain near Conistone lake. Very perfect quadrangular prisms of argillaceous schistus are found near Llanwrst. Rubble slate assumes the columnar form at Barmouth. The limestone near Cyfartha, in Glamorgan-shire, is divided into very regular acute rhomboidal prisms: even the sandstone of the same district is not unfrequently columnar; and one of the beds of gypsum at Montmartre is distinctly divided into pretty regular columns. Sandstone, clay, argillaceous iron ore, and many other substances, become prismatic by torrefaction; and the prisms of starch formed in drying have often been considered as illustrative of basaltic formations.

Mere contraction may produce effects resembling these, but less regular.

I am very far from conceiving, that all these configurations are influenced by such systematic arrangements as have determined the form of some basaltic columns. I consider most of them as solely attributable to contraction; which is only a farther extension of the aggregative force, and must be regulated by the texture, the form, and the position of the mass. Where the texture of the mass is homogeneous, and its contractions uniform, its dimensions may be diminished, without its continuity being destroyed, provided its aggregation be so strong as to overcome the *vis inertia* of the mass, and its adhesion to other substances. But, when the resistance is sufficient to overcome the aggregation, the mass will be rent by fissures perpendicular to the direction in which the greatest resistance to its contraction takes place, or, in other words, by fissures perpendicular to its greatest surface: for it is from the extremities of the greatest surface, that the largest quantity of

Developement of the effects of contraction in producing symmetric fractures.

perfect prisms; but the most beautiful I have seen, are the small ones from Ponza. The columns at Bolsena are said to be basalt. Those of the Euganean hills are very irregular in form; in their texture they are certainly wholly unlike granite, which Mr. Strange thought they resembled. I believe them to be lava.

The mention of some columnar formations that follows, is by no means intended as an enumeration of them. I have confined myself to those which I have either inspected in their natural situation, or of which I have seen numerous specimens.

matter must traverse the greatest space, in order that the contraction may be performed without breach of continuity; therefore, if it be an extensive tabular mass, it will be divided into prisms, by fissures perpendicular to its surfaces. The power of aggregation would determine these prisms to be hexagonal, as that form contains the greatest quantity of matter in the least surface, of any prisms that can be united without interposing prisms of other forms. But this would require the texture, the contraction, the thickness of the mass, and its adhesion to surrounding substances, to be every where precisely the same; and, as these conditions can never be fulfilled in an extensive formation, all the irregularities that are found must necessarily ensue. The same rule that determines the fissures of a tabular mass to be perpendicular to its surfaces, must determine the rents in a spheroid to be directed from its periphery to its centre.

Though these considerations may be sufficient to explain the tendency to division into prisms, which is so generally extended, and which has produced many of those abortions that have been dignified with the name of columns, because they have occurred in lavas and in rocks of trap formation, they are utterly inadequate to illustrating the formation of the more perfect basaltic prisms: they offer no means of accounting for the extreme regularity of the sides and the precision of the angles, for the articulations, for the close contact in which the perfect columns are placed to one another, not for their mutual adhesion, which is so strong, that it often requires considerable violence to separate them. These facts are in absolute contradiction to all idea of retreat or contraction, and seem to me to coincide perfectly with the explanation of their origin which I have already presumed to lay before you.

They do not account for the regular form of basaltic.

I have the honour to be, &c.

GREGORY WATT.

VI.

Improved Construction of Woulfe's Apparatus. By W. B.

To Mr. NICHOLSON.

SIR,

Improvement in Woulfe's apparatus. **E**VERY one who has been in the habit of using a Woulfe's apparatus, must have experienced the difficulty of fixing the tubes of communication, and the continual risk of breaking them. To obviate this inconvenience, I would suggest the following mode of constructing the apparatus, which, if you think any improvement upon the present method, cannot be rendered more generally useful than by insertion in your Journal.

I am, Sir,

Your very obedient humble servant,

W. B.

Feb. 5, 1805.

Fig. 1 represents one of the bottles. The bent tube, A B C, being attached to the outside of the bottle when both heated, and entering it at C, may be made as strong as is thought consistent with neatness.

The part H B of the tube is of a conical form, small enough at A to be inserted in a perforated cork.

The short tube D E, on the opposite side, is also of a conical form, large enough at E to admit a perforated cork.

The opening at F being intended for the tube of safety.

Fig. 2 represents the apparatus put together, the receiver being added as of a convenient form for connecting with the other vessels.

The small tube A B of one bottle, is inserted in the perforated cork fitted to the tube D E of the preceding one, and the joinings secured in the usual way by luting.

The tubes of safety being first securely fixed, and the cork closely fitted in the tube D E, any number of bottles may be successively added, without the least risk of deranging those that are already connected.

Obstruction

VII.

Observations on the Change of some of the proximate Principles of Vegetables into Bitumen; with Analytical Experiments on a peculiar Substance which is found with the Borey Coal. By CHARLES HATCHETT, Esq. F. R. S. From the Philosophical Transactions for 1804.

§ I.

ONE of the most instructive and important parts of geology, is the study of the spontaneous alterations by which bodies formerly appertaining to the organized kingdoms of nature have, after the loss of the vital principle, become gradually converted into fossil substances. Conversion of organized bodies into fossils.

In some cases, this conversion has been so complete, as to destroy all traces of previous organic arrangement; but, in others, the original texture and form have been more or less preserved, although the substances retaining this texture, and exhibiting these forms, are often decidedly of a mineral nature. Some, however, of these extraneous fossils (as they are called) retain part of their original substance or principles, whilst others can only be regarded as casts or impressions. The degrees are various.

From the animal kingdom we may select, as examples, the fossil ivory, which retains its cartilage*; the bones in Gibraltar rock, consisting of little more than the earthy part or phosphate of lime; the shells forming the lumachella of Bleyberg, which still possess the lustre and iridescence of their original nacre; and the shells found at Hordwell in Hampshire, and in Picardy, which are chiefly porcellaneous, but more or less calcined; also the fossil echini and others, so commonly found in the limestone, chalk, and calcareous grit of this island, which, although they retain their original figure, are intirely, or at least externally, formed of calcareous spar, incrusting a nucleus of flint or chalcedony. And if, in addition to these, we may be allowed to regard the more recent limestone and chalk strata as having been principally or partly formed from the detritus of animal exuviae, we shall possess a complete series of gradations, commencing with animal substances analogous in Instances in the animal kingdom,

* I have also found the cartilage perfect, in the teeth of the mammoth.

properties to those which are recent, and terminating in bodies decidedly mineral, in which all vestiges of organization have been completely destroyed.

and in the vegetable.

The vegetable kingdom has likewise produced many instances not less remarkable; and it is worthy of notice, that animal petrifications are commonly of a calcareous nature, while, on the contrary, the vegetable petrifications are generally siliceous*.

It is not, however, my intention here to enter into a minute discussion concerning the formation of these extraneous fossils; I shall therefore proceed to consider other equally or perhaps more important changes, which organized bodies, especially vegetables, appear to have suffered, (after the extinction of the principle of life,) by being long buried in earthy strata, and by being thus exposed to the effects of mineral agents.

§ II,

Bituminous matters are derived from organized matters; chiefly vegetables.

The principal object I have in view, is to adduce some additional proofs, that the bituminous substances are derived from the organized kingdoms of nature, and especially from vegetable bodies; for, although many circumstances seem to lead to the opinion, that the animal kingdom has in some measure contributed to the partial formation of bitumen, yet the proofs are by no means so numerous, nor so positive, as those which indicate the vegetable kingdom to have been the grand source from which the bitumens have been derived. But this opinion, (founded upon very strong presumptive evidence,) although generally adopted, is however questioned by some persons; and I shall therefore bring forward a few additional facts, which will, I flatter myself, contribute to demonstrate, that bitumen has been, and is actually and immediately formed, from the resin, and perhaps from some of the other juices of vegetables.

Pure bitumens differ much from vegetable juices;

The chemical characters of the pure or unmixed bitumens, such as naptha, petroleum, mineral tar, and asphaltum, are, in certain respects, so different from those of the resins and other inspissated juices of recent vegetables, that, had the former never occurred but in a separate and unmixed state, no positive inference could have been drawn from their pro-

* Pyrites, ochraceous iron ore, and fahlertz, are also occasionally found in the forms of vegetable bodies.

parties

perties in proof of their vegetable origin. Fortunately, however, they have been more frequently found under circumstances which have strongly indicated the source from whence they have been derived; and much information has been acquired from observations made on the varieties of turf, bituminous wood, and pit coal, on the nature of their surrounding strata, on the vestiges of animal and vegetable bodies which accompany them, and on various other local facts; all of which tend considerably to elucidate the history of their formation, and to throw light upon this interesting part of geology.

Some instances have already been mentioned, which show that fossil animal substances form a series, commencing with such as are scarcely different from those which are recent, and terminating in productions which have totally lost all traces of organization.

Similar instances are afforded by the vegetable kingdom; but without entering into a minute detail of every gradation, I shall only cite three examples in this island, namely,

1. The submarine forest at Sutton, on the coast of Lincolnshire, the timber of which has not suffered any very apparent change in its vegetable characters *. Series of change.
1. Submarine wood.

2. The strata of bituminous wood (called Bovey Coal) found at Bovey, in Devon; which exhibits a series of gradations, from the most perfect ligenous texture, to a substance nearly approaching the characters of pit coal, and, on that account, distinguished by the name of Stone Coal. 2. Bovey coal in all its gradations.

3. And lastly, the varieties of pit coal, so abundant in many parts of this country, in which almost every appearance of vegetable origin has been destroyed. 3. Perfect pit-coal.

The three examples above-mentioned, appear to form the extremities and centre of the series; but as, from some local circumstances, the process of carbonization, and formation of bitumen, has not taken place in the first instance, and as these effects have proceeded to the ultimate degree in the last, it seems most proper that we should seek for information, and for positive evidence, in the second example, which appears to be the mean point, exhibiting effects of natural operations, by which bitumen and coal have been imperfectly and partially

* Account of a submarine Forest on the East Coast of England, by Dr. Correa de Serra. Phil. Trans. for 1799, p. 145.

formed, without the absolute obliteration of the original vegetable characters; and, although I have selected the Bovey coal as an example, because it is found in this country, we must recollect that similar substances, or strata of bituminous wood, are found in many parts of our globe; so that the example which has been more immediately chosen, is neither rare nor partial*.

Remarkable
schistus,

The nature, however, of the various kinds of bituminous wood, may in some respects be different; but this I have not as yet had the means of ascertaining; I shall therefore only state the facts resulting from experiments made on Bovey coal, and more especially on a peculiar bituminous substance with which it is accompanied. But, before I enter into these particulars, it will be proper to mention a very remarkable schistus, with which I was, some months since, favoured by the Right Hon. Sir Joseph Banks.

§ III.

from Iceland,

This schistus was found by Sir Joseph, in the course of his tour through Iceland, near Reykum, one of the great spouting hot springs, distant about twenty-four English miles from Hafnifjord; but circumstances did not permit him to ascertain the extent of the stratum.

consisting very
much of alder
leaves.

The singularity of this substance is, that a great part of it consists of leaves, which are evidently those of the alder, interposed between the different lamellæ. I do not mean mere impressions of leaves, such as are frequently found in many of the slates, but the real substance, in an apparently half charred state, retaining distinctly the form of the leaves, and the arrangement of the fibres.

External cha-
racters.

The schistus is light, brittle, of easy exfoliation, in the transverse fracture earthy, and of a pale brown colour; but, when longitudinally divided, the whole surface constantly presents a series of the leaves which have been mentioned, uniformly spread, and commonly of a light gray on the upper surface, and of a dark brown on the other; the fibres on the

* Strata of bituminous wood are found in various parts of France, in the vicinity of Cologne, in Hesse, Bohemia, Saxony, Italy, and especially in Iceland, where it is known under the name of Surturbrand.

light gray surface being generally of a blackish-brown, which is also the colour assumed by the schistus when reduced to powder.

The leaves appeared to be in the state of charcoal, by being extremely brittle, by the blackish brown colour, by deflagrating with nitre, by the manner of burning, and by forming carbonic acid. I was, however, soon convinced that the substance of these leaves was not complete charcoal, but might more properly be regarded as vegetable matter in an incipient state of carbonization, which, although possessed of many of the apparent properties of charcoal, still retained a small portion of some of the other principles of the original vegetable.

The leaves almost in the state of coal;

My suspicion was excited, partly by the odour produced during combustion, which rather more resembled that of wood than that of charcoal, and partly by the brown solution formed by digesting the powder of the unburned schistus in boiling distilled water; for, by various tests I ascertained, that the substance thus dissolved was not of a mineral nature. In order, however, fully to satisfy myself in this respect, I digested 250 grains of the pulverized schistus with six ounces of water.

but burning with the odour of wood.

Digestion of the schistus in water, and examination of the solution.

The liquor was, as before, of a dark brown colour.

It had but little flavour.

Prussiate of potash, muriate of barytes, and solution of isinglass, did not produce any effect; nitrate of silver formed a very faint cloud; sulphate of iron was slowly precipitated, of a dark brownish colour; and muriate of tin produced a white precipitate.

A portion of the solution, by long exposure to the air, was partially decomposed; and a quantity of a brown substance was deposited, which could not again be dissolved in water.

Another portion was also evaporated to dryness, and afforded a similar brown substance, which was only partially soluble in water; and the residuum, in both of the above cases, was found to be insoluble in alcohol, and in ether.

When burned, it emitted smoke, with the odour of vegetable matter.

250 grains of the schistus, afforded about three grains of the above substance; and, when the properties of the aqueous solution are considered, such as its partial decomposition, and the deposit which it yielded by exposure to air, and by evaporation;

tion; the insolubility of this deposit when again digested with water, alcohol, or ether; the smoke and odour which it yielded when burned; and the precipitates formed by the addition of sulphate of iron and muriate of tin to its solution; when these properties, I say, are considered, there seems much reason to conclude, that the substance dissolved by water was vegetable extract, which had apparently suffered some degree of modification, but not sufficient to annul the more prominent characteristic properties of that substance.

The powder of the schistus, which had been employed in the preceding experiment, was afterwards digested in alcohol during two days; and a pale yellow tincture was thus formed, which, by evaporation, left about one grain of a yellow transparent substance, possessing the properties of resin.

It contained vegetable extract, and a little resin,

but no tannin.

It appears, therefore, that a substance very analogous to vegetable extract, and a small portion of resin, remain inherent in the leaves of this remarkable schistus.

As solution of isinglass did not produce any effect, there was reason to conclude, that the aqueous solution above-mentioned did not contain any tannin; but, as the tannin might be combined with the alumina of the schistus, I digested a portion of it in muriatic acid, which, after filtration, was evaporated almost to dryness, leaving, however, the acid in a slight excess. This was diluted with water; and afforded a blue precipitate with prussiate of potash, a yellowish precipitate with ammonia, and a white precipitate with muriate of tin, but not any with solution of isinglass. The tannin which might have been contained in the recent vegetable, appears therefore to have been dissipated or decomposed, with the greater part of the other vegetable principles, excepting the woody fibre reduced to the state of an imperfect coal, and the small portions of extract and resin which have been mentioned.

Previous to having made the analysis, I had an idea, that this schistus might be a lamellated incrustation, formed by the tufa of the hot springs; but, according to Mr. Klaproth's analysis*, the tufa of Geyser is composed of,

Silica	-	-	98
Alumina	-	-	1.50
Iron	-	-	50

100.

* *Beiträge; Zweiter band, p. 109.*

It is therefore very different from the schistus, the component ingredients of which were ascertained from the following analysis.

ANALYSIS OF THE SCHISTUS FROM ICELAND*.

A. 250 grains, by distillation, yielded water, which, in the latter part of the process, became slightly acid and turbid, Analysis of the schistus.
= 42.50 grains.

B. The heat was gradually increased, until the bulb of the retort was completely red hot. During the increase of the heat, a thick brown oily bitumen came over, which weighed 7.50 grains; it was attended with a copious production of hydrogen, carbonated hydrogen, and carbonic acid, the whole of which may be estimated at 23.75 grains.

C. The residuum was black, like charcoal, and weighed 176.25 grains; but, being exposed to a strong red heat in a crucible of platina, it burned with a faint lambent flame, and was at length reduced to a pale brown earthy powder, which weighed 122 grains; so that 54.25 grains were consumed.

D. The 122 grains were mixed with 240 of pure potash; and, as some particles of charcoal remained, 50 grains of nitre were added, and the whole was strongly heated, during half an hour, in a silver crucible. The mass was then dissolved in distilled water, and, muriatic acid being added to excess, the liquor was evaporated to dryness, and was again digested with muriatic acid much diluted; a quantity of pure silica then remained, which, after having been exposed to a red heat, weighed 98 grains.

E. The liquor from which the silica had been separated, was evaporated nearly to dryness, and added to boiling lixivium of potash; after the boiling had been continued for about one hour, the liquor was filtrated, and a quantity of oxide of iron was collected, which amounted to 6 grains.

F. Solution of muriate of ammonia was added to the preceding filtrated liquor; and, the whole being then heated, a copious precipitate of alumina was obtained, which, after having been made red-hot, weighed 15 grains.

Carbonate of soda caused the preceding liquor (after the separation of alumina) to become slightly turbid, but not any precipitate could be collected.

* The remaining specimens are now in the British Museum, and in the collection of the Right Hon. Charles Greville.

Component
parts.

By this analysis, 250 grains of the schistus afforded,

		Grains.
Water	- - - A.	42.50
Thick brown oily bitumen	} B.	{ 7.50
Mixed gas (by computation).		
Charcoal (by computation)	C.	54.25
Silica	- - - D.	98
Oxide of iron	- - E.	6
Alumina	- - - F.	15

247.

But the water and vegetable matter must be regarded as extraneous; and, if they are deducted, the real composition of the schistus is nearly as follows.

Its component
parts, exclusive
of water and
vegetable matter.

Silica	- - -	82.30
Alumina	- - -	12.61
Oxide of Iron	- - -	5.

99.91.

It evidently, therefore, belongs to the family of argillaceous schistus, although the proportion of silica is more considerable than has been found in those hitherto subjected to chemical analysis.

The schistus has not been noticed by von Troil, nor by any of those who have written concerning Iceland; for the slate which was sent to Professor Bergmann by the former, and which is mentioned by the latter in one of his letters, is there expressly stated to be the common aluminous slate containing impressions*.

§ IV.

Question,
whether the
circumstances of
the production
of this schistus
were peculiar.

From the experiments which have been related, we find that the leaves contained in the Iceland schistus, although they are

* Letters on Iceland, by Uno von Troil, p. 355.

Mr. Faujas St. Fond has however described a schistus nearly similar, which is found near Roche-Seauve, in the Vivarais. The stratum extends about two leagues; and the only difference is, that, according to Mr. St. Fond, the schistus at Roche-Seauve is of the nature of marble, or, as he terms it, argillo-calcareous, whereas this of Iceland is undoubtedly argillaceous. From Mr. St. Fond's account, it does not appear that the vegetable leaves contained in the schistus of Roche-Seauve have been chemically examined. *Essai de Geologie*, par B. Faujas St. Fond, Tome I. pp. 128 and 134.

apparently

apparently reduced almost to the state of charcoal, nevertheless retain some part of their original proximate principles, namely, extract and resin. This, of itself, is undoubtedly a remarkable fact; but, if it were unsupported by any other, the only inference would be, that the schistus was most probably of very recent formation, and had been produced under peculiar circumstances.

I was desirous, therefore, to discover some similar cases, which might serve as additional corroborative proofs of the gradual alterations by which vegetable bodies become changed, so as at length to be regarded as forming part of the mineral kingdom; and, from the reasons which have been stated in the commencement of this paper, as well as from a certain similarity in the external characters of the substance composing the leaves above-mentioned with those of the Bovey coal, I was induced to make this last also a subject of chemical inquiry.

In the Philosophical Transactions for the year 1760*, some Bovey coal. remarks on the Bovey coal, and an account of the strata, are stated, in a letter from the Rev. Dr. Milles to the Earl of Macclesfield. The object, indeed, of the author, was to establish that this and similar substances are not of vegetable, but of mineral origin; and, to prove this, he adduces a great number of cases, most of which, however, in the present state of natural history and of chemistry, must be regarded as proving the contrary; whilst others, mentioned by him, such as the Kimmeridge or Kimendge coal, are nothing more than bituminous slates, and of course are of a very different nature.

Dr. Milles's account of the varieties of the Bovey coal, and its history. of the state of the pits at that time, appears to be very accurate; and, for the present state, or at least such as it was in 1796, I shall beg leave to refer to a paper of mine, published in the fourth volume of the Transactions of the Linnean Society †; for, as this is more immediately a chemical investigation, I wish to avoid, as much as possible, entering into any minute detail of geological circumstances.

* Vol. LI. p. 534.

† Observations on bituminous Substances, p. 138. See also Parkinson's Organic Remains of a former World. Vol. I. p. 126.

As strata agree
with those of
Surturbrand.

It may however be proper to observe, that the Bovey coal is found in strata, corresponding in almost every particular with those of the Surturbrand in Iceland, described by Von Troil*, and by Professor Bergmann†. The different strata of both these substances are likewise similar, being composed of wood or trunks of trees, which have completely lost their cylindrical form, and are perfectly flattened, as if they had been subjected to an immense degree of pressure‡.

The

* Von Troil's Letters, p. 42.

† *Opuscula Bergmanni*, Tom. III. *De Productis Volcaniis*, p. 239.

‡ Bergmann, in the dissertation above quoted, accurately describes this appearance of the Surturbrand, and then says, "Quæ autem immanis requiritur vis, ut truncus cylindricus ita complanetur? Nonne antea particularum nexus putredinis quodam gradu fuerit relaxatus? Certe, nisi compages quodammodo mutatur, quodlibet pondus incumbens huic effectui erit impar. Ceterum idem observatur phenomenon in omni schisto argillaceo." This is certainly a very curious fact; and the learned Professor, with his usual acuteness, rejects the idea that mere weight can have been the cause. As a farther proof also, he afterwards observes, "Orthoceratitæ, quæ in strato calcareo conicam figuram perfectè servant, in schisto planum fere triangulare compressione efficiunt. Idem valet de pilcibus, conchis, insectisque petrefactis." And again, "Observatu quoque dignum est, quod idem reperitur effectus, quamvis stratum calcareum sub schisto collocatum sit, et majori ideo pondere compressione onustum." *De Productis Volcaniis*, p. 240. It is evident, therefore, that weight alone has not produced this effect; and Bergmann's idea, that the solidity of the vegetable bodies may have undergone some previous change, in the manner of incipient putrefaction, by moisture, and by becoming heated in the mass, must be allowed to be very probable. But bodies such as shells could not be thus affected; and therefore they must have been exposed to some mechanical effect, peculiar to argillaceous strata; which effect, however, from the circumstances which have been adduced, evidently could not have resulted from the mere pressure of the superincumbent strata. To me, therefore, it seems not very improbable, that, together with a certain change in the solidity of vegetable bodies, produced in the manner imagined by Bergmann, and, together with some degree of superincumbent pressure, a real and powerful mechanical action has been exerted, by the contraction of the argillaceous strata, in consequence of

The Bovey coal is commonly of a chocolate-brown, and sometimes almost black. The quality and texture of it are various in different strata; from some of these, it is obtained in the form of straight flat-pieces, three or four feet in length, resembling boards, and is therefore called Board Coal. Others have an oblique, wavy, and undulating texture, and, as Dr. Milles observes, have a strong resemblance to the roots of trees, from which, most probably, this sort has in a great measure been formed.

External characters of Bovey coal.

Some kinds also appear to be more or less intermixed with earth; but that which produces the most powerful and lasting fire, is called stone coal; it is black, with a glossy fracture; has little or none of the vegetable texture; is more solid and compact than the others, being almost as heavy as some of the pit coals, the nature of which it seems very nearly to approach.

For chemical examination, I selected some of the coal which had a wavy texture, and rather a glossy fracture; the quality of this sort being apparently intermediate between the others, as it retains completely the marks of its vegetable origin, while, at the same time, it possesses every perfect character of this species of coal.

A. 200 grains of the Bovey coal, by distillation, yielded, Chemical examination of Bovey coal.

					Grains.
1. Water, which soon came over acid, and afterwards turbid, by the mixture of some bitumen	-	-	-	-	60
2. Thick brown oily bitumen	-	-	-	-	21
3. Charcoal	-	-	-	-	90
4. Mixed gas, consisting of hydrogen, carbonated hydrogen, and carbonic acid,	} estimated at				29
					<hr/> 200.

The charcoal, in appearance, perfectly resembled that which is made from recent vegetables. By incineration, about 4 grains of yellowish ashes were left, which consisted of alumina, iron, and silica, derived most probably from some small portion of the clay strata which accompany the Bovey coal.

of deficcation; this, I believe, has not hitherto been much considered, but I am inclined, from many circumstances, to attribute to it a very great degree of power.

But

But it is very remarkable, that neither the ashes obtained from the charcoal of the Bovey coal, nor those obtained from the leaves of the Iceland schistus, afforded the smallest trace of alkali.*

B. 200 grains of the Bovey coal, reduced to powder, were digested in boiling distilled water, which was afterwards filtrated, and examined; but I could not discover any signs of extract, or of any other substance.

C. 200 grains were next digested with six ounces of alcohol, in a very low degree of heat, during five days. A yellowish-brown tincture was thus formed, which, by evaporation, afforded a deep brown substance, possessing all the properties of resin, being insoluble in water, but soluble in alcohol, and in ether; it also speedily melted, when placed on a red-hot iron, burned with much flame, and emitted a fragrant odour, totally unlike the very unpleasant smell produced by burning the coal itself, or by burning any of the common bituminous substances. The quantity, however, which could be extracted from 200 grains of the coal, by alcohol, was but small, as it did not exceed 3 grains. But this small quantity was sufficient to prove, that although the Bovey coal does not contain any vegetable extract, like the schistus formerly mentioned, yet the whole of the proximate principles of the original vegetable have not been entirely changed; as a small portion of true resin, not converted into bitumen, still remains inherent in the coal, although the bituminous part is much the most prevalent, and causes the fetid odour which attends the combustion of this substance.

It resembles the schistus in its products as well as character; but it afforded no extract, but only resin;

Upon a comparison of the general external characters of the Bovey coal, with those of the substance which forms the leaves contained in the Iceland schistus, a very great resemblance will be observed; and this is farther confirmed, by the similarity of the products obtained from each of them in the preceding experiments, with the single exception, that the leaves contain

* This, as far as relates to the Bovey coal, has been also noticed by Dr. Milles, Phil. Transf. Vol. LI. p. 553. But wood, however long submerged, is not deprived of alkali, unless it has more or less been converted into coal; for I have, since the reading of this paper, made some experiments on the wood of the submerged forest at Sutton, on the coast of Lincolnshire, and have found it to contain potash.

some

some vegetable extract, which I could not discover in the Bovey coal. They agree however in every other respect; as they both consist of woody fibre in a state of semicarbonization; impregnated with bitumen, and a small portion of resin, perfectly similar to that which is contained in many recent vegetable bodies; and thus it seems, that as the woody fibre, in these cases, still retains some part of its vegetable characters, and is but partially and imperfectly converted into coal, so, in like manner, some of the other vegetable principles have only suffered a partial change. Undoubtedly, there is every reason to believe that, next to the woody fibre, resin is the substance which in vegetables passing to the fossil state, most powerfully resists any alteration; and that, when this is at length effected, it is more immediately the substance from which bitumen is produced. The instances which have been mentioned corroborate this opinion; for the vegetable extract in one of them, and more especially the resin which was discovered in both, must be regarded as part of those principles of the original vegetables which have remained, after some other portions of the same have been modified into bitumen.

which most probably is that which affords the bitumen.

The smallness of the quantity of resin obtained in both the preceding cases, by no means invalidates the proof of the above opinion; but, as an additional confirmation of it, I shall now give an account of a very singular substance, which is found with the Bovey coal.

§ V.

Dr. Milles, in his remarks on the Bovey coal, (which I have several times had occasion to notice in the course of this Paper,) states, that “amongst the clay, but adhering to the coal, are found lumps of a bright yellow loam, extremely light, and so saturated with petroleum, that they burn like sealing wax, emitting a very agreeable and aromatic scent.”*

Singular substance found with the Bovey coal.

This substance, I also observed, when I visited the Bovey coal-pits, in 1794 and 1795. At that time, however, it was scarce, and I could only procure one small specimen, which is now in the British Museum; but, from a cursory examination of it, I was convinced that it was a peculiar bituminous substance,

It is a peculiar bitumen.

* Phil. Trans. Vol. LI. p. 535.

and not loam impregnated with petroleum, as Dr. Milles had supposed. I could not then conveniently make a regular analysis of it, and therefore contented myself with briefly describing it, in a note annexed to my Paper on bituminous Substances. *

Lately, however, my friend John Sheldon, Esq. of Exeter, F. R. S. obligingly sent me several pieces of it, together with specimens of the different kinds of Bovey coal which have been mentioned; and thus I was enabled fully to ascertain its real nature and properties.

DESCRIPTION OF THE BITUMEN FROM BOVEY.

Description of
the bitumen
from Bovey.

It accompanies the Bovey coal, in the manner already described, and is found in masses of a moderate size.

The colour is pale brownish ochraceous yellow.

The fracture is imperfectly conchoidal.

It appears earthy externally, but, when broken, exhibits a slight degree of vitreous lustre.

The fragments are irregularly angular, and completely opaque at the edges.

It is extremely brittle.

It does not apparently become softened, when held for some time in the hand, but emits a faint resinous odour.

The specific gravity, at temperature 65° of Fahrenheit, is, 1,135.

Some specimens have dark spots, slightly approaching in colour and lustre to asphaltum; and small portions of the Bovey coal are commonly interspersed in the larger masses of this bitumen.

When placed on a heated iron, it immediately melts, smokes much, burns with a bright flame, and yields a very fragrant odour, like some of the sweet-scented resins, but which at last becomes slightly tainted with that of asphaltum.

The melted mass, when cold, is black, very brittle, and breaks with a glossy fracture.

EXPERIMENTS.

Experiments on
the same.

A. 100 grains of this bitumen, when distilled until the bulb of the retort became red-hot, afforded,

* Transactions of the Linnean Society, Vol. IV. p. 139.

	Grains.
1. Water slightly acid - - - - -	3
2. Thick brown oily bitumen, very similar to that which was obtained from the Bovey coal, but possessing slightly the odour of vegetable tar. - - - - -	48
3. Light spongy coal - - - - -	23
4. Mixed gas, composed of hydrogen, carbonated hydrogen, and carbonic acid, (by computation,) - - - - -	29.

The coal yielded about three grains and a half of ashes, which consisted of alumina, iron, and silica, with a trace of lime.

B. The bitumen was not affected by being long digested in boiling distilled water.

C. By digesting 100 grains in lixivium of pure potash, a brown solution was formed; this was saturated with muriatic acid, and a brown resinous precipitate was obtained, which weighed 21 grains.

D. A portion was digested in nitric acid: at first, much nitrous gas was evolved, and, after the digestion had been continued for nearly 48 hours, a part was dissolved, and formed an orange-coloured solution, which did not yield any precipitate, when saturated by the alkalis, or by lime; the colour only became more deep, and, by evaporation, a yellow viscid substance was obtained, which was soluble in water. The above nitric solution possessed every property of those nitric solutions of resinous substances which I have mentioned in a former paper.*

E. The benzoic and succinic acids were not obtained from this substance, by any of the methods usually employed.

F. Alcohol almost immediately began to act upon this bitumen; and, being added at different times, gradually dissolved a considerable part of it. The solution was reddish-brown, and has a resinous odour; by the addition of water it became milky, and, by evaporation, afforded a dark brown substance, which had every property of resin, whilst the residuum left by the alcohol possessed those properties which characterize asphaltum.

The following analysis was then made, to discover the proportions of the component ingredients.

* Phil. Trans. for 1804, p. 198.

ANALYSIS OF THE BITUMEN FROM BOVEY.

Analysis of the
bitumen from
Bovey.

A. 100 grains, reduced to a fine powder, were digested during 48 hours with six ounces of alcohol, the vessel being placed in sand moderately warmed. A deep reddish-brown tincture was thus obtained; and the operation was again twice repeated, with other portions of the same menstruum, until it ceased to act upon the residuum.

The whole of the spirituous solution (which had been cautiously decanted) was then subjected to a very gradual distillation in an alembic, and yielded a brown fragrant resin, which weighed 55 grains.

B. The residuum, which could not be dissolved by alcohol, was digested in boiling distilled water, but this did not act upon it; the whole was therefore collected on a filter, was gradually dried, without heat, by mere exposure to the air, and then weighed 44 grains.

These 44 grains consisted of a light, porous, pale-brown substance, which, being melted, formed a black, shining, brittle mass. It burned with the odour of asphaltum, but rather less disagreeable, owing most probably to a small portion of the resin, which had not been completely extracted by the alcohol. It was insoluble in water, and in alcohol, but was readily dissolved by heated fat oils; and in every other particular was found to possess the properties of asphaltum.

The 44 grains of asphaltum, when burned, left a residuum, which weighed three grains, and consisted of alumina, silica, and iron.

By this analysis it appears, that the bitumen which accompanies the Bovey coal, is a peculiar and hitherto unknown substance, which is partly in the state of vegetable resin, and partly in that of the bitumen called asphaltum, the resin being in the largest proportion, as 100 grains of the above-mentioned substance afforded,

Its component
parts.

Resin	-	-	-	-	-	55
Asphaltum	-	-	-	-	-	41
Earthy residuum	-	-	-	-	-	3
						<hr/> 99.

Thus we have an instance of a substance being found under circumstances which constitute a fossil, although the characters of it appertain partly to the vegetable, and partly to the mineral kingdom.

§ VI.

The powerful action which alcohol exerts on most of the ^{Experiments on} resins, may justly be regarded as forming a marked distinction ^{bitumens by the} between those substances and the bitumens. But, as some of ^{action of alco-} the bitumens are acted upon by alcohol, in a slight degree, I was desirous to ascertain whether a small portion of resin was contained in any of these; or, if that was not the case, I wished to determine the nature of the substances which could be separated, although very sparingly, by this menstruum. I therefore made the following comparative experiments, on the soft brown elastic bitumen from Derbyshire; on the genuine asphaltum; on very pure cannel coal; and on the common pit coal.

100 grains of each were digested with three ounces of alcohol, in matrasses placed in warm sand, during five days, some alcohol being occasionally added, to supply the loss caused by evaporation. After the above-mentioned period had elapsed, the liquid contained in each matrass was poured into separate vessels.

I. The alcohol which had been digested on the elastic bitu- ^{Results:} men was not tinged, nor, when spontaneously evaporated, did it leave any film or stain on the glass.

II. From asphaltum, the alcohol had extracted a yellow tincture, which, in some situations, appeared of a pale olive colour, and, being spontaneously evaporated, a thick brown liquid was deposited, in small drops, on the glass; these drops did not become hard after two months, and possessed the odour, and every other property, of petroleum. The asphaltum had lost in weight about one grain and a half.

III. The cannel coal had communicated a pale yellow tint to the alcohol, which, in the manner above described, was ascertained to be caused by petroleum; but, from the smallness of the quantity, the weight could not be determined.

IV. The alcohol which had been digested on pit coal, had not assumed any colour; but, by spontaneous evaporation*, it left a film on the glass, which, by its odour, was also found to be petroleum.

By these experiments we find, that the action of alcohol on ^{Alcohol acts} the bitumens is very slight; and that the small portion which ^{slightly on bitu-} ment.

* Spontaneous evaporation, by exposure to the air, was employed in these experiments, for reasons which must be sufficiently obvious.

may

may thus be extracted from some of them, is petroleum. In these, the process of bituminization (if I may be allowed to employ such a term) appears to have been completed, whilst in the Bovey coal, and especially in the substance which accompanies it, nature seems to have performed only the half of her work, and, from some unknown cause, to have stopped in the middle of her operations. But, by this circumstance, much light is thrown on the history of bituminous substances; and the opinion, that they owe their origin to the organized kingdoms of nature, especially to that of vegetables, which hitherto has been supported only by presumptive proofs, seems now, in a great measure, to be confirmed, although the causes which operate these changes on vegetable bodies are as yet undiscovered.

They are not
formed by time
alone.

Many facts indicate, that time alone does not reduce animal or vegetable bodies to the state of fossils. In this country, there are numerous examples of large quantities of timber, (even whole forests) which have been submerged prior to any tradition, and which nevertheless completely retain their ligneous characters.* Other local causes and agents must therefore have been required, to form the varieties of coal and other bituminous substances. In some instances (as in the formation of Bovey coal), these causes seem to have acted partially and imperfectly, whilst, in the formation of the greater part of the pit coals, their operation has been extensive and complete.

In the pit coals, the mineral characters predominate, and the principal vestige of their real origin seems to be bitumen; for the presence of carbon in the state of oxide, cannot alone be considered as decisive.

Bitumen, therefore, with the exuviae and impressions so commonly found in the accompanying strata, must be more immediately regarded as the proofs, in favour of the origin of pit coal from organized bodies; and, considering the general facts which have been long observed, together with those lately adduced respecting the Bovey coal, and the substance which is found with it, we seem now to have almost unques-

* Phil. Transf. for January, 1671. Phil. Transf. Vol. XIX. p. 526. Ibid. Vol. XXII. p. 980. Ibid. Vol. XXIII. p. 1073. Ibid. Vol. XXVII. p. 298. Ibid. for 1799, p. 145.

tionable evidence, that bitumen has essentially been produced by the modification of some of the proximate principles of vegetables, and especially resin.

Modern chemistry had comparatively made but a small progress, when the illustrious Bergmann published his Dissertation, entitled, *Producta Ignis subterranei chemice considerata*; for, at that time, the extent and power of chemical action, in the humid way, were very imperfectly understood. In that part, however, of the above work, where he speaks of the fossil wood of Iceland, called Surturbrand, he evidently appears doubtful how far volcanic fire may have acted upon it; although he conceives that, in the formation of it there has been some connection with volcanic operations. His words are, "*Quid de ligno fossili Islandiæ sentiendum sit, gnaro in loco natali contemplatori decidendum relinquimus. Interea, ut cum vulcani operationibus nexum credamus, plures suadent rationes, quamvis hucusque modum ignoremus, quo situm texturamque adquisiverunt hæc strata.*" Whether they be of igneous or aqueous formation. It certainly was very natural that Bergmann should entertain this opinion, in respect to the surturbrand; and it is remarkable, that the leaves contained in the schistus lately described, are of the same nature, and are found in the same country. The leaves also described by Mr. St. Fond, are likewise found in a country which, according to him, was formerly volcanic. Were these substances, therefore, never found but in countries which either actually are or were volcanic, we should be almost compelled to believe, with the Swedish professor, that the operations of subterraneous fires have been concerned in the formation of these bodies, or rather in the conversion of them into their present state.

But similar substances are found in countries where not the smallest vestige of volcanic effects can be discovered, and Devonshire most undoubtedly is such; yet, nevertheless, the Bovey coal is there found similar to the surturbrand, in most of the external, and (from experiments which I made some years ago, I believe I may say) chemical properties; to which must be added, that both these substances perfectly resemble each other, by forming regular strata.* They are found in countries evidently not volcanic.

* Transf. of the Linnean Society, Vol. IV. p. 138. Von Troil's Letters, p. 42. *Opuscula Bergmanni*, Tom. III. p. 239.

Moreover,

Deduction.

Moreover, the half charred appearance of Bovey coal, and of furturbrand, cannot be adduced as any proof, that the original vegetable bodies have been exposed to the partial effects of subterraneous fire; for, at this time, we know that the oxidizement of substances is performed, at least as frequently, and as effectually, by the humid as by the dry way. It would therefore be superfluous here, to enter into an elaborate discussion, to prove that coal and bitumen, with much greater probability, have been formed without the intervention of fire; and I am the less inclined to say more upon this subject, as I have already published some considerations on it in a former paper.*

The new substance found with the Bovey coal termed *Retinasphaltum*.

Before I conclude, I must beg leave to observe, that as the substance which is found with the Bovey coal is, in every respect, so totally different from any of the bitumens hitherto discovered, it seems proper that it should receive some specific name; and, as it has been proved to consist partly of a resin and partly of a bituminous substance, I am induced to call it *Retinasphaltum*, † a name by which a full definition of its nature is conveyed.

Account of another specimen, probably of the same.

I have lately seen, in No. 85 of the *Journal des Mines*, p. 77, an account of a peculiar combustible fossil, found near Helbra, in the county of Mansfield, and described by Mr. Voight, in his *Versuch einer Geschichte der Steinkohle, der Braunkohle, &c.* p. 188. This substance is of an ash-coloured gray, passing to grayish-white; it is found in a bed of bituminous vegetable earth, which has apparently been produced by the decomposition of fossil wood. The purest specimens are in the form of nodules; the fracture is earthy; it is opaque; soft; brittle; and is very light. When applied to the flame of a candle, it burns and melts like sealing-wax, at the same time diffusing an odour which is not disagreeable. This substance appears to accord in so many properties with the *retinasphaltum* of Bovey, that I cannot but suspect it to be of a similar nature, and I have little doubt that, by a chemical examination, it will be found to consist partly of resin and partly of bitumen.

* Trans. of the Linnean Society, Vol. IV. pp. 141, &c.

† From *resin*, resin; and *ασφαλτος*, bitumen.

VIII.

Account and Description of an improved Air Pump. By Mr.
N. MENDELSSOHN.

To Mr. NICHOLSON.

SIR,

ON entering into business, as a mathematical instrument-maker, I resolved to make it my principal study to introduce ^{Introductory letter.} into them all the improvements which the present advanced state of science, the nature of the subject, and my humble abilities would allow. Thus I have begun with improving *Volta's electrical lamp* and the *air-pump*. I imagine, I have rendered the construction of the latter much more simple than it usually is, and consequently less liable to derangement, at the same time that it possesses a very great exhausting power. Give me leave, Sir, to offer you a description of this improved air-pump with the requisite drawings. It is ready for your inspection. I shall think myself very happy, if this useful philosophical instrument meet with your approbation. And if it be so fortunate, I request you the favour of introducing it into public notice, by inserting my paper in your excellent and deservedly celebrated Journal.

I am, with the highest esteem, Sir,

Your humble and obedient servant,

N. MENDELSSOHN.

No. 50, *Surry-Street, Black-Friars,*
Feb. 13, 1805.

Notwithstanding the many improvements which have been made upon the construction of the air-pump, it was still desirable that this instrument should be more simplified in its mechanism, and yet possess the same advantages of those constructed lately. That the mechanical power of the pump, and not the pressure of the air, should open the valves, has long been required and already done by Mr. Cuthbertson, Mr. Haas, and several other skilful artists; but may I be allowed to remark, that on reading the description of their instruments, they appeared to me to be too compounded. It must be very difficult
for

*Description of
Mendelsohn's
improved air-
pump.*

Description of
Mendelsohn's
improved air-
pump.

for an experimental philosopher to clean an instrument which, being thus complicated, is not only rendered intricate, but is also difficult to be put together again. Being desirous, therefore, to simplify this instrument, I adopted the construction here described, by which it is capable of being put together in less than half an hour when cleaned, and requires that operation very seldom.

I submit to the judgment of your scientific readers how far the present instrument answers its desired purpose. I have rejected that tube which, in common air-pumps, leads from the valves to the receiver, together with the cock that serves to shut this pipe: the receiver is placed immediately upon the valves, these being put on the top of the cylinders, which, consequently, required the rackwork and pinion to be underneath, and inverted the whole instrument. *See the adjoining drawing, Plate VI.* where A B and C D represent the two cylinders of glass ground and polished inside. E and F are the two valves that allow the cylinders to communicate with the receiver O through two very short canals A B and C D (*Fig 2, Plate VII*) and the cock G. Two other valves that open into the atmosphere are within the covers *i* and *k*, as may be seen in *Fig. 1*, where *c* represents one of them. M N is the receiver-plate of glass ground flat; P Q a barometer-gauge, upon the plan of the first Torricellian tube, as the easiest to construct and the most infallible in its effects. It will be found to be here quite out of the way, secure from being broke by accident, and the most in sight. H K and I L are two brass pillars that support the whole. R S V W the usual rackwork, having a double winch *l m*, which, upon trial, will be found preferable to a single one.

It will now be necessary to shew how this pump acts, in which it will be sufficient to explain the action of one cylinder, because the other is in all parts alike. E is a conical metallic valve, from which a canal goes through the cock G up to the receiver, as is seen in *Fig. 1* and 2, *Plate VII.* where all the parts are marked with the same letters. E T is a steel rod going through a leather box in the piston U. The top of this rod is fixed to the valve E, and its bottom part slides in a small hole with an allowance of 0,1 inch up and downward, consequently the valve E can move no farther. When the piston descends, it first opens the valve by pushing the rod to the

the bottom of the hole. Then it slides down along the rod E T, and the air from the receiver has now free access to the cylinder. When the piston returns it lifts the rod E T, and thus shuts up the valve. Then the piston slides again along the rod up to the top of the cylinder, condensing the air above it, which air, by the least condensation, opens a valve *c*, Fig. 2, and escapes freely into the atmosphere. This last valve has neither spring or additional weight to shut it, but shuts by its own weight (about a quarter of an ounce) as soon as the piston is arrived to the top of the cylinder.

The cylinders are made of glass, and the pistons of tin, so well fitted as to be air-tight, without the interposition of any leathers. The friction of these two bodies is small beyond expectation, a sufficient proof that they will be durable. They possess the further advantage of being capable of standing for even six months, after which time they will serve without being cleaned or repaired, because they are not liable to be corroded by the oil which they contain, an inconvenience too general in brass cylinders. After all, if the present pump should want cleaning, it is an easy operation to take off the top piece *gh*, by unscrewing the nuts H and I, when this piece, with all the apparatus upon it, will come off. Then each cylinder may very easily be slid off from the piston, wiped out and replaced, after having greased its inside with a little of the cleanest sweet oil: The top is then to be put again in its place, and the two nuts H and I being screwed upon it, the instrument is ready. Neither racks or pinion need to be taken out of their places, the cylinders standing above them.

The cock is constructed so, that, being in the situation represented in Fig. 1, the communication is open between the cylinders, the receiver, and the barometer-gauge, and, by a quarter of a revolution, the cylinders are excluded, the receiver and gauge being still left in communication. A little stopper in Fig. 3, ground into the cock, being open, air is admitted to the receiver if required.

The receiver-plate is of glass ground flat, as was mentioned before: this will be found preferable to brass, because cleaner, and never corroded by acids or water; it will besides often prove very convenient in making experiments on electricity in the vacuum.

The

The whole instrument is fixed upon a mahogany table, which serves as a stand to it.

I will conclude by observing, that neither the employing of glass cylinders, or the method of opening the valves, are new, but, for aught I know, this is the first instrument of the kind ever executed in this country; as likewise the idea of putting the valves at top, and thus simplifying the instrument, seems to have escaped the attention of the eminent artists both here and abroad; as to my best knowledge, it has never been done or described any where. The metallic pistons, without leathering, must certainly add to the durability, and diminish the great labour that usually attends working an air-pump.

IX.

Letter concerning Palladium, from WILLIAM HYDE WOLLASTON, M. D. F. R. S. the Discoverer of that Metallic Body.

To Mr. NICHOLSON.

SIR,

The author adverts to former proceedings respecting palladium; of which he is the discoverer.

THE candour with which you communicated all circumstances that came to your knowledge concerning palladium, at a time when the discoverer of that substance was yet unknown to you, demands my earliest acknowledgments, as having been the author of those communications; and it is proper that I should also express the satisfaction I received on learning the respectable tribunal you nominated at my request, for examining the merits of any attempts that might be made to form that substance artificially.

Reasons why the discovery was first communicated to the world anonymously.

As I have already shewn (in a paper which you did me the honour to reprint in your Journal for January last, p. 34.) by what means a very small quantity of Palladium may be extracted from the ore of platina, and as I have there examined the synthetic attempts to prove that this body was a compound, with a degree of attention which I thought due to the chemical skill of the person who proposed them, as well as to the degree of uncertainty that must attend a subject entirely new; I cannot now adduce further chemical evidence, and can only add, for the information of those whose judgment has been biased by the difficulty of accounting for the production

duction of so large a quantity of palladium as was offered for sale, that a proportional quantity of platina, from which the whole was extracted, was purchased by me a few years since, with the design of rendering it malleable for the different purposes to which it is adapted. That object has now been attained, and during the solution of it, various unforeseen appearances occurred, some of which led me to the discovery of palladium; but there were other circumstances which could not be accounted for by the existence of that metal alone. On this, and other accounts, I endeavoured to reserve to myself a deliberate examination of those difficulties which the subsequent discovery of a second new metal, that I have called rhodium, has since enabled me to explain, without being anticipated even by those foreign chemists, whose attention has been particularly directed to this pursuit,

I remain, Sir,

Your obliged and obedient Servant,

W. H. WOLLASTON.

Feb. 23, 1805.

X.

Short Remark on Mr. Walker's last Letter respecting Focal Images. By C. L.

To Mr. NICHOLSON.

SIR,

I WOULD beg your indulgence for a very short letter, in answer to Mr. Ezekiel Walker. The only reply which seems ^{C. L. declines any farther discussion respecting Mr. Walker's experiments with lenses; but denies the facts.} needful, on the subject of the temper in which he or I may have written, is that if his first paper had indicated more of the calm spirit of philosophy, as well as of philosophical correctness, my observations might either have been unnecessary, or, perhaps, drawn up without any extraneous remark. As it is, I do not think myself entitled, nor am I indeed inclined to offer any strictures upon his last; which I leave to the unbiassed judgment of your readers. On the present occasion, I only wish to adhere to the experiments. Men of science will find no difficulty in forming a proper estimate of our reasonings. In plain language, therefore, I will beg leave

to wave any attention to the history of Mr. Walker's experiments, upon which I have animadverted, and to deny his facts. What may have deceived him in his proceedings, his admeasurements, and his repeated registerings, is not for me to discuss. If he will send his lenses to you, and you should find that, under any circumstances whatever of distance or position, it be possible, by a mere alteration in the aperture, to produce a difference, as 2 to 3, in the length of the focal image, (see *Philos. Journal*, Vol. IX. p. 165.) I think it will be incumbent upon us to re-examine all the facts and demonstrations, of what we have hitherto been in the habit of calling the science of optics, in order to reconcile them with so strange a result.

I am, Sir,

Your obliged

C. L.

XI.

A Communication on the Use of Green Vitriol, or Sulphate of Iron, as a Manure; and on the Efficacy of paring and burning depending, partly, on Oxide of Iron. By GEORGE PEARSON, M. D. Honorary Member of the Board of Agriculture, F. R. S. From a Communication made by him to the Board, and inserted in the fourth Volume of their Transactions.

Sulphate of iron
or martial vitriol
hitherto supposed
destructive of
vegetation,

I TAKE leave to lay before this Honourable Board, an account of a substance as a manure, which I find, on examination, is one of the things, hitherto universally believed to be a poison to vegetables. Having ascertained that this substance is what is commonly known by the name of vitriol of iron (the sulphate of iron of the chemists), inveterate opinion prevented me for some time from accepting the testimony of it as a manure; but feeling the weight of the respectable evidence by whom it was attested, after consideration I perceived that the fact in question was not at variance with established principles of vegetable philosophy, as I shall, I think, make appear in this communication.

—but the contrary is true.

My friend John Williams Willaume, Esq. of Tingrith in Bedfordshire, having desired his brother, Charles Dymoke Willaume, Esq. to ask my opinion of a saline substance collected

lected from peat, which has been used with profitable consequences as a manure in his neighbourhood; I proposed a set of queries to Mr. John W. Willaume, the answers to which, in the two following copied letters, comprehend the evidence I have to offer.

LETTER No. I.

To Dr. Pearson, from C. D. Willaume, Esq.

MY DEAR SIR,

I RECEIVED the inclosed last Saturday, and hope the answers to your queries will be satisfactory, and tend to elucidate this curious subject. Though the answers under the article *dust* only relate to your queries, yet my brother has thought proper to advert to the *ashes*, which you conceive to be a *caput mortuum*; but which have been used as, and have been supposed to be, a beneficial manure from time immemorial. I have reserved a piece of the peat from which the ashes are produced, and if you would wish to analyse it, I will send it you. Favour me with the result of your future inquiries on this subject, and I am,

My dear Sir, yours very sincerely,

Walham Green,
Aug. 24, 1801.

C. D. WILLAUME.

LETTER No. II.

From John W. Willaume, Esq. to C. W. Willaume, Esq.

Queries proposed by Dr. Pearson.

1. How long has the salt of peat been used?
2. How much per acre is laid on?
3. On what kind of lands?
4. The effects of it on vegetation?
5. Whether it is mixed with dung manure, or lime?
6. In what parts of the country has it been employed?
7. Any other facts which can be collected relative to the use of this substance?

Queries respecting salt of peat.

In answering the above queries, I shall divide the subject into three articles, 1st, the *peat* considered as an object of fuel; 2d, the *ashes*; 3d, the *salt of peat, or dust*; the two last as objects of manure.

1. *Peat*

Description of
peat.

1. *Peat.* The peat, which is found after the removal of the turf or exterior surface, to about a spade's depth, has long been known as an article of fuel. It is, however, used only by cottagers, who burn it on a brick hearth; it has been rejected from the parlour, the kitchen, the brewhouse, &c. as being injurious to grates, and to all sorts of vessels put on it; it cannot be employed in the roasting of meat, as it will impart a disagreeable taste, and it is destructive of all sorts of furniture by the effluvia which it emits, or by the dust or ashes which may chance to be blown from it. If these disagreeable consequences could be obviated, it might be made an article of general consumption as a substitute for coal, much to the advantage of the seller and consumer; it is dug out in the form of a brick to a certain depth, well known to the common labourer. This depth must be carefully attended to, lest you should cut out the staple, in which case it would never be retrieved; but, this circumstance attended to, it will grow again to its former state in the space of fifteen years. Thus the whole moor is divided into proper portions, and periodically cut once in fifteen years.

Its ashes.

2. *Ashes.* The turf or surface, and such parts of the peat as do not appear to be of the best quality, are laid up in considerable heaps, and reduced to ashes by the action of fire. *The ashes are red.*

Answer to Queries.

Answers to the
queries as to the
ashes.

1. The ashes have been long known as a manure, and the demand is on the increase.

2. The quantity usually laid on an acre, by spreading or sowing it, is fifty bushels, either on grass or arable land.

3. It is laid on hot land. By hot land, we understand sandy, gravelly, chalky soils of a dry nature, such as are burnt up on the long continuance of hot weather. It is most commonly used for grasses; but is in considerable esteem, as a manure, for oats or barley on land of the nature above-mentioned.

4. The vegetable effect is surprising, inasmuch as it will double or treble a crop of any new sown grass, such as trefoil, &c. I have seen the benefits arising from it on old pasture land much overgrown with moss, which it effectually destroys, and produces in its stead white or Dutch clover. You may trace

trace to an such the cessation and recommencement of this manure. It is observable, that near the fire heaps, as far as the wind can carry the lighter parts of the ashes, the production of clover is sure to be abundant; it is equally favourable to the growth of barley or oats.

5. It is not mixed with lime, or any other manure.

6. These ashes are bought, by a set of higlers, who carry them in bags loaded on asses to a considerable distance, where they are known to be in great repute; they must come exceedingly dear to the consumer by this mode of conveyance. The farmers in the vicinity send for them in waggons, particularly Mr. Brumiger, near Sundon in Bedfordshire, a considerable and intelligent farmer, who increases his consumption every year, both for his grass and arable land.

3. The Salt of Peat, or Dust.

Answer to Queries.—1. The dust or gray saline substance is produced by beating the earth containing this salt to a powder; it is found in particular spots, not universally, the earth not being equally impregnated with it in all places; it has not been known as a manure above six years; but on trial greatly increases in reputation and demand. Answers respecting the salt of peat.

2. Fifty bushels are the proper quantity per acre. This should not be exceeded, for if it be laid on in too great abundance, it may prove extremely deleterious.

3. It is used for cold lands. By cold lands we understand clayey, or any wet grounds.

4. It will much improve the vegetation of sowed grasses, and old pasture, and is equally favourable to the production of corn; the ground, whether grass or arable, being of a cold nature.

5. It is not mixed with lime, or any other substance.

6. The dust is likewise bought by the higlers, and carried to great distances. The nearer farmers likewise send for the dust in waggons, particularly Mr. Anstie, of Dunstable Houghton, and Mr. Smith, of Sundon, who hold this manure in great esteem.

Your's, &c.

Tingrith, Aug. 19, 1801.

J. W. WILLAUME.

Dr. Pearson's Experiments, Observations, and Remarks on the Substance called Salt of Peat, or Dust.

Dr. Pearson's
chemical exami-
nation of salt of
peat or dust.

1. It is a blackish gray, coarse, and rather heavy powder. Has no smell; tastes strongly styptic; readily dissolves in the mouth; did not deliquesce on exposure to the air.

2. Dissolves in four times its weight of water of the temperature of sixty degrees of Fahrenheit, and in twice its weight of boiling hot water, giving a pale green coloured solution, with a trifling sediment, which is insoluble in muriatic acid.

3. To the solution (2.) I added a little liquid prussiate of vegetable alkali in a perfectly neutral state, which occasioned immediately a most abundant precipitation of prussiate of iron; and this test was added gradually, till no further precipitation took place.

4. Into the decanted and filtrated fluid (3) was poured liquid caustic volatile alkali, but without inducing any change.

5. Into the same fluid (3) was poured liquid carbonate of vegetable alkali, which produced a scarcely perceivable cloudy appearance.

6. Into the solution (3) was dropped the aqueous solution of muriate of baryt, which occasioned immediately a milky appearance.

7. To the solution (3) I added the oxalic acid, and turbidness ensued.

8. A little of the powdery substance, called the salt of peat, with concentrated sulphuric acid, produced no emission of fumes, nor smell.

9. The solution (2) with muriate of baryt, immediately grew thick and white as cream.

10. The solution (2) with carbonate of potash, deposited a very copious greenish sediment; and the same effect ensued with caustic volatile alkali.

11. The solution (2) with oxalic acid, gave instantly a very turbid bluish green precipitation.

The preceding experiments manifested that the *peat salt* consists of *sulphate of iron*, vulgarly called green vitriol of iron, mixed with a very minute proportion of silicious earth, and of lime united either to sulphuric acid, or to carbonic acid. But the presence of the earths magnesia and argill; the uncombined

bined alkalies; the uncombined acids; are by these experiments excluded. In short, the salt of peat is almost pure *sulphate of iron*.

Remarks.

1. The salt of peat is, I apprehend, deposited by evaporation which run over the moors, where it is found; and hence I should expect many of such waters to be strongly impregnated with it, and in many parts the soil to be tinged red and yellow by ochre. Very likely * on enquiry much iron pyrites will be found on, or near the moors.

2. The quantity spread on land is said to be fifty bushels per acre, which I estimate at 2,250 pounds avoirdupoise; this will give near seven ounces and a half per square yard. If a larger quantity be applied, it is observed it will prove extremely deleterious. This is true also of every other manure, such as lime, alkaline salts, marine salt, nay, of the dung of animals: for if they be used in certain quantities, they *poison* plants, instead of promoting their growth. This is equally true in the animal kingdom; for there is not an article taken as food, or as seasoning, which is not a poison, if taken in certain quantities. A human creature may be poisoned or alimented by beef or pudding, according to the quantity of them taken into the stomach. He may be poisoned, or have digestion greatly assisted by salt, or pepper, according to their quantity. In brief, the vulgar notion of the term *poison* is erroneous: for by it is conceived that substances so called are in their nature positively destructive of life; but the truth is that the most virulent poisons are, in all reason and fact, only deleterious according to the quantity applied. White arsenic swallowed in the quantity of ten grains or less, will destroy life; but in the quantity of one-sixteenth of a grain, it is as harmless as a glass of wine; and further, in that dose is a remedy for inveterate agues.

From these considerations I conclude, that there is no admissible contradictory evidence to the testimonies for the fer-

* "This is," says Mr. Willaume, "exactly the fact. This sulphate of iron, the salt of peat, during the heat of the summer is frequently found in a crystallized state, very white, and crackling under the feet; but is deliquescent in that form, and turns to its former dark colour when the air becomes moist."—*Not by Mr. J. W. Williams.*

utilizing effect of sulphate of iron, unless by such contravening evidence the quantity stated to be used exceed fifty bushels per acre; it being an established fact, that in certain proportions this metallic salt is a poison to plants.

This discovery of Mr. Willaume, will, I think, give new light, so as to explain fully the *rationale* of the improvement of land by the burnt earth and ashes from piring and burning. It is usual to account for the effects of this process, by referring to supposed alkaline or other salts; but of these there is no evidence, nay, on trial I have not detected them, or at least not in any efficient quantity; but this I know, that such earth and ashes contain *oxide of iron*, and as I suspect of *manganese*; which from the analysis, and the effect of salt of peat, must now be admitted into the class of manures. This very communication of Mr. Willaume, affords evidence of the truth of this conjecture, for the *ashes of the peat which afford the salt* "have been long known as a manure, and the demand is on the increase:" of course, ~~these~~ these ashes contain an unusual quantity of oxide of iron. A consequence of this reasoning is, that the burnt earth of soils will, *ceteris paribus*, fertilize in proportion to the oxide of iron it contains. Accordingly the ashes of the peat, says Mr. Willaume, have a surprising effect, they "will double or treble a crop of any new-sown grass, such as trefoil, &c." they are so beneficial, that in spite of the expence they are carried in bags by higlers to great distances. It would be extending this paper beyond the proposed limits, to reason at a greater length, and to make a further induction of facts: therefore I will close with asserting, that the more I contemplate the facts in Mr. Willaume's letter, the more evidence I perceive for the truth, that metallic salts, and metallic oxides in general, and salts and oxides of iron in particular, are manures, if applied in proper doses.

It is considered as effective upon the same principle as condiments or seasoners.

I do not think it is within the design of this paper to make observations on the answers to the 2d, 3d, 4th, 5th, and 6th queries, except, once, for all desiring that it may be understood, that I consider the *salt of peat and the ashes of peat*, as operating in promoting vegetation analogous to seasoning, or condiments, taken with the food of animals; that is, analogous to mustard, cinnamon, ginger, &c. which are not of themselves at all or necessarily nutritious, but contribute to render other things nutritious, by exciting the action of the stomach and

and other organs of digestion and assimilation. I have no doubt of the truth of the proposition, that no living thing, neither plant nor animal, can grow and live in a state of visible action without constant supplies of *matter which has been alive*; in other words, *living* animals and vegetables can only live on *dead* animals and *dead* vegetables. No plant, nor animal has ever been known by experience, nor in the nature of things does it seem reasonable, that they can be nourished by mere water and pure air, as some persons have asserted.

I shall make a few remarks on the other *two substances* which are the subject of Mr. Willaume's letter.

2. The Peat.

The peat is a dense mass of vegetable matter for a certain depth, partly in a *dead* and partly in a *living* state, with which is mixed more or less earth, and in burning it affords so much empyreumatic oil, as to give a disagreeable taste to roasted provisions; hence, as we are told, it has been rejected from the kitchen. This fuel affords a vast quantity of what the chemists call *lignic acid*; hence it is rejected also from the parlour, as very destructive to the grates. I beg to suggest that this lignic acid might be saved in burning the peat as fuel, and be used for various purposes in manufactures; and the charred peat may be used in place of charcoal of wood. Probably too other useful products will be found, on examining the matters more accurately which are afforded by distillation.

3. Ashes.

If the peat were mere vegetable matter, the ashes afforded by it would be as trifling as those of wood; but some parts of the moor contain so much earth and oxide of iron, as to leave behind, on burning, a considerable quantity of incombustible matter; and such kind of peat, we are told, is not used as fuel; but, after burning, the residuary matter is an efficacious manure, much more so than is commonly afforded by paring and burning. The ashes are more red and more fertilizing than ashes of common turf, because they contain more iron.

The spontaneous springing up of white clover, in land manured with these ashes, is similar to the spontaneous growth of this plant on heath land, which has been covered with lime to destroy all its present vegetation; and this fact shows that probably

Explanation.

bably these are seeds buried in the earth for many ages, which yet remain alive, but do not grow until exposed to the stimuli of air, water, caloric, and lifeless animal, or vegetable matter.

(To be concluded in our next.)

XII.

Letter from Mr. ACCUM, respecting an Error stated to exist in his Practical Chemistry.

To Mr. NICHOLSON.

SIR,

Compton-Street, Soho,
Feb. 16, 1805.

Omission stated to be made in Accum's chemistry.

VV. F. C. censures me in your Journal, No. 38, page 105, of not having given in my book on Practical Chemistry "*The means of preparing either nitric acid, or nitrate of potash, or rather having stated them by implication, as incapable of being produced by art.*" The fidelity with which you have laid his remarks before the public, encourages me to hope, that with equal impartiality you will allow me to appear before the same tribunal, in order to plead to this accusation NOT GUILTY. For the method of obtaining nitric acid from its constituent principles, W. F. C. may read in the Vol. I. page 211.

In order to let the judicious readers who are not in possession of the work, judge for themselves, I beg leave to lay before them the method there pointed out, which literally runs thus :

Passage where the supposed omission is found.

"Take a barometer tube, the diameter of which is about $\frac{1}{2}$ part of an inch. Shut one of its extremities with a cork, through the middle of which passes a small wire with a ball of metal at each end. Fill the tube with mercury and invert it into a basin of this fluid. Throw up into this tube as much of a mixture of 12 parts of nitrogen gas, and 87 parts of oxygen gas as will fill 3 inches. Through this gas by means of the wire in the cork pass a number of electric sparks; the volume of the gas gradually diminishes, and in its place will be found nitrous acid." *

* Nitrous acid does not differ from nitric acid, in composition, but merely by the admixture of nitrous gas. See the book under consideration, Vol. II. p. 288.

It

It appears that W. F. C. has not noticed these lines. The production of nitric acid by synthesis, being here clearly stated, his misadversion relative to that subject falls to the ground.

To exculpate myself from his further accusation, namely, of not having noticed the production of nitrate of potash by artificial means, or rather having stated it by implication, will become equally erroneous, on reminding him, that the method of preparing this salt artificially is pointed out, Vol. II. page 287, thus, "*Nitrate of potash may be prepared by neutralizing carbonate of potash with nitric acid.*" Other explanations.

From what has been stated, it is obvious that W. F. C.'s remarks are not correct, for both the methods of obtaining nitric acid, and nitrate of potash are correctly given, and not by implication, as W. F. C. apprehends.

I have the honour to be, Sir,

Your's,

FREDERICK ACCUM.

XIII.

Processes for obtaining a durable and superior Lake from Madder.

*By Sir H. C. ENGLEFIELD, M. P. F. R. S.**

THE want of a durable red colour, which should possess something of the depth and transparency of the lakes made from cochineal, first induced me to try whether the madder root, which is well known to furnish a dye less subject to change by exposure to air, than any other vegetable colour, except indigo, might not produce something of the colour I wanted. Introduction.

Several of the most eminent painters of this country have, for some time, been in the habit of using madder lakes in oil pictures: but the colours they possessed under this name were either a yellowish red, nearly of the hue of brick-dust, or a pale pink opaque, and without clearness or depth of tint, and quite unfit to be used in water-coloured drawing, which was the principal object of my search. Madder lakes have been already used; but they were bad.

My first attempts were to repeat the process given by Margraf, in the Memoirs of the Academy of Berlin: but the process affords a weak colour, Margraf's process affords a weak colour,

* From the Transactions of the Society of Arts, who voted the gold medal in honour of this discovery. 1804.

lour

lour produced by this mode was of a pale red, and very opaque, although the eminent author of the process states the colour he produced to be that of "*le sang enflammé*," which probably means a deep blood colour. It may, however, be observed, that colours prepared with a basis of alumine will appear much deeper when ground in oil than they do in the lump, the oil rendering the alumine nearly transparent. This advantage is however, lost in water-colours. On examining the residuum of the madder root, after it had been treated in Margraf's method, it appeared tinged with so rich a red, that it was obvious,

because most of the colour remained in the root.

that by far the greater part of the colour still remained in it, and that the most powerful and beautiful part. To extract this, several ineffectual trials were made, which it would be useless to enter into; but, on attentively examining the appearances which took place on infusing the madder in water, I began to suspect that the red colouring matter was very little, if at all, soluble in water, and that it was only mechanically mixed with the water when poured on the root, and suspended in it by the mucilage, with which the root abounds.

Water takes up little of the colour.

Whence mechanical pressure is advisable.

A very small quantity, therefore, can be obtained by any infusion or decoction, as the greater part sinks down on the root, or remains with it on the sieve, or in the bag, through which the infusion or decoction is passed to render it clear. I therefore was induced to try whether, by some merely mechanical means, I could not separate the colouring matter from the fibrous part of the root. In this attempt my success was fully equal to my hopes; and, after several trials, I consider the process I am now about to describe, as the most perfect I have been able to discover.

Process 1.
Two ounces of madder were pounded in a bag with a pint of water;

Process 1. Enclose two ounces, troy weight, of the finest Dutch madder, known in commerce by the name of crop madder, in a bag, capable of containing three or four times that quantity, and made of strong and fine calico. Put it into a large marble, or porcelain mortar, and pour on it about a pint of cold soft water. The Thames water, when filtered, is as good as can be used; it being very nearly as pure as distilled water, at least when taken up a very little way above London. With a marble or porcelain pestle, press the bag strongly in every direction, and, as it were, rub and pound it as much as can be done without endangering the bag. The water will very soon be loaded with the colouring matter, so

as to be quite opake and muddy. Pour off the water, and add another pint of fresh water to the root, agitating and triturating it in the manner before described; and repeat the operation till the water comes off the root very slightly tinged. About five pints of water, if well agitated and rubbed, will extract from the root nearly the whole of its colour; and if the residual root be taken out of the bag and dried, it will be found to weigh not more than five drachms, apothecaries weight; its colour will be a kind of light nankeen, or cinnamon, and it will have entirely lost the peculiar odour of the root, and only retain a faint woody smell.

The water when turbid with colour is poured off and another pint added; and rubbed as before:

Five successive pints extract all the colour, leaving five drams of fibre.

The water loaded with the colouring matter, must be put into an earthen or well-tinned copper, or, what is still better, a silver vessel, (for the use of iron must be carefully avoided through the whole), and heated till it just boils. It must then be poured into a large earthen or porcelain basin, and an ounce and one oz. of alum dissolved in about a pint of boiling soft water, must be poured into it, and stirred until it is thoroughly mixed. About an ounce and a half of a saturated solution of mild vegetable alkali should be gently poured in, stirring the whole well all the time. A considerable effervescence will take place, and an immediate precipitation of the colour. The whole should be suffered to stand till cold; and the clear yellow liquor may then be poured off from the red precipitate. A quart of boiling soft water should again be poured on it, and well stirred. When cool, the colour may be separated from the liquor by filtration through paper in the usual way; and boiling water should be poured on it in the filter, till it passes through of a light straw colour, and quite free from any alkaline taste. The colour may now be gently dried; and when quite dry, it will be found to weigh half an ounce; just a fourth part of the weight of the madder employed.

The coloured water is made boiling hot;

and one oz. of alum dissolved in a pint of boiling water added; $1\frac{1}{2}$ oz. of mild veg. alk. afford a precipitate of colour.

Edulcoration, filtering and drying afford half an ounce of lake,

By analysis, this colour possesses rather more than 40 per cent. of alumine. If less than an ounce of alum be employed with two ounces of madder, the colour will be rather deeper; but if less than three quarters of an ounce be used, the whole of the colouring matter will not be combined with alumine. On the whole, I consider the proportion of an ounce of alum to two ounces of madder, as the best.

which contains two fifths of alumine.

Process 2. If, when the solution of alum is added to the water loaded with the colouring matter of the root, the whole

Process 2. If the liquor of process 1. be suffered to cool

and settle after the alum was added it affords a dull red sediment, and the remaining fluid if heated and precipitated by alkali, affords a fine but not solid lake. This is Watt's process.

be suffered to stand, without the addition of the alkali, a considerable precipitation will take place, which will be of a dark dull red. The remaining liquor, if again heated, will, by the admission of the alkali, produce a rose-coloured precipitate of a beautiful tint, but wanting in force and depth of tone.

This is the process recommended by Mr. Watt, in his Essay on Madder, in the *Annals de chimie*, Tome 7; and this latter colour is what may perhaps, with propriety, be called Madder Lake. But, although the lighter red may be excellent for many purposes, yet I consider the colour produced by the union of the two colouring matters, as given in the first process, as far preferable for general use, being of a very beautiful hue when used thin, and possessing unrivalled depth and richness either in oil or water, when laid on in greater body.

Less alum causes the second precipitate to be less in quantity, but richer.

If but half an ounce of alum be added to the two ounces of the root, the first precipitate will be nearly similar to that when an ounce is employed; but the second, or lake precipitate, will be less in quantity, and of a deeper and richer tint. In this case the whole of the colouring matter, as before observed, is certainly not combined with the alumine; for, on adding more alum to the remaining liquor, a precipitate is obtained of a light purplish red. In this process, when two ounces of madder and an ounce of alum are used, the first precipitate has about 20 per cent. of alumine, and the second, or lake precipitate, about 53 per cent; but these proportions will vary a little in repetitions of the process.

Process 3. When the washing is performed by boiling instead of cold water, the colour is neither so good nor so plentiful.

Process 3. If the madder, instead of being washed and triturated with cold water, as directed in the foregoing process, be treated in exactly the same manner with boiling water; the colour obtained will be rather darker, but scarcely of so good a tint; and the residuum of the root, however carefully pressed and washed, will retain a strong purplish hue; a full proof that some valuable colour is retained in it, probably fixed in the woody fibre by the action of heat. Mr. Watt, in his excellent treatise on madder above mentioned, observes, that cold water extracts the colour better than hot water; and I have reason to suspect, that a portion of that colouring matter, which produces the bright red pigment, distinguished before by the name of Madder Lake, remains attached to the root, when acted on by boiling water.

Process 4.

Process 4. If to two ounces of madder, a pint of cold water be added, and the whole be suffered to stand for a few days (three or four days) in a wide-mouthed bottle, lightly corked, in a temperature of between 50° and 60°, and often shaken; a slight fermentation will take place, the infusion will acquire a vinous smell, and the mucilaginous part of the root will be in a great degree destroyed, and its yellow colour much lessened. If the whole be then poured into a calico bag, and the liquor be suffered to drain away without pressure, and then the root remaining in the bag be treated with cold water, &c. exactly as directed in the first process, the red colouring matter will quit the root with much greater ease than before fermentation. It will also be equal in quantity to that afforded by the first process, but of a much lighter red. This difference of tint appears to be owing to a destruction of a part of the lake by the fermentation of the root; for if the colours from the fermented root be obtained separate, as in Process 2, the first precipitate will not sensibly differ from that obtained from the unfermented madder, but the second, or lake will be of a very light pink. This process, then, is not to be recommended.

Process 4. If the madder be suffered to stand before washing in cold water, the mucilage is destroyed by fermentation, and the colour more easily extracted; but it is not so good.

Spanish and Smyrna Madders.

Spanish Madder affords a colour of rather a deeper tone than the Dutch Madder, but it does not appear to be of so pure a red as the Zealand Crop Madder.

Spanish madder affords a deeper but less pure colour than Dutch madder. Smyrna madder is better than Dutch.

The Smyrna Madder is a very valuable root. The colour produced from it by Process 1, is of a deeper and richer tint than any I have obtained from the Dutch Madder. The quantity produced from two ounces, is only three drachms, twenty-four grains: but this is not to be wondered at; for as this madder is imported in the entire root in a dry state, and the Crop Madder of Zealand consists principally of the bark, in which probably the greatest part of the colouring substance resides, there is every reason to think that the Smyrna madder really contains a greater proportion of colour than the Zealand, in equal weights of the entire root.

The products of Process 2 prove, that the lake of the Smyrna madder is more abundant in quantity, and of a richer tone than that of the Dutch root; for, from two ounces of Dutch madder the first precipitate was two drachms, and the lake was two drachms and forty eight grains; whereas, from

two ounces of the Smyrna root the first precipitate was one drachm and twenty four grains, and the lake was two drachms and twenty-four grains. The proportion of the lake to the other colour is, therefore, much higher in the Smyrna, than in the Dutch root.

Fresh Madder.

The recent root is preferable.

The colour may be prepared from the recent root; and it will be of a quality equal, if not superior, to any other. The difficulty of procuring the fresh root has prevented me from making as many experiments on it as I could have wished. I procured, however, a small quantity of the best roots packed in moss from Holland, and the following process answered perfectly well.

Experiment according to process 1. with fresh madder;

Eight ounces of the root having been first well washed and cleaned from dirt of all kinds, were broken into small pieces, and pounded in a bell-metal mortar, with a wooden pestle, till reduced into an uniform paste. This paste being enclosed in a calico bag, was washed and triturated, as described in the first process, with cold water. About five pints seem to have extracted nearly the whole of the colour. To the water thus loaded with colour, and boiled as before, one ounce of alum, dissolved in a pint of boiling water, was added, and the alkali poured on the whole, till the taste of the mixture was just perceptibly alkaline. The colour thus obtained, when dry, was of a very beautiful quality.

It promises great advantage to the arts.

The success of this experiment, which was twice repeated with the same result, has led me to hope, that it is not impossible that the mode of obtaining the colour from the fresh root here described, may be productive of advantages for more extensive use than I had in view when first I attempted to obtain a pigment from madder. Many tracts of land in this country are as well adapted to the growth of this valuable article, as the soil of Holland can be; and the cultivation of it, which has more than once been attempted to a considerable extent, has been laid aside, principally from the expence attendant on the erection of drying-houses and mills, and the great expence and nicety requisite for conducting the process of drying. But should the colour prepared in the mode just described, be found to answer the purposes of the dyers and calico-printers, the process is so easy, and the apparatus required

For if the colour should answer for dying, &c. it would save drying and carriage;

quitted

quired for it so little expensive, that it might be in the power of any grower of the root to extract the colour: besides which, another great advantage would be obtained; the colour thus separated from the root, may be kept any length of time, without danger of spoiling, and its carriage would be only one fourth of that of the root. I am, moreover, thoroughly inclined to believe, that in the present mode of using the root, a very considerable part of the colour is left in it by the dyers; and, should this prove to be the case, an advantage much greater than any hitherto adverted to, may arise from the process here recommended.

may be kept for a long time,

and probably be more effective.

Should it be attempted to obtain the colour from the fresh root, on an extensive scale, I should recommend, that the root be first reduced to as uniform a pulp as possible, by grinding or pounding. To this purpose, it is probable that the cyder-mill would answer perfectly well; and its extreme simplicity is a great recommendation. For the purpose of trituration, bags of woollen, such as are used in the oil-mills, would probably answer as well as calico, and they would be much cheaper and more durable. A large vat, with stampers, would be easily constructed, by those who are conversant in mechanics, for the holding them and pressing them in water; and when the colour was boiled and precipitated, the flues of the boilers might easily be formed into convenient drying tables, without any additional expense of fuel. The part of the process, which I consider as of the greatest importance, and as being the essential advantage of my methods over all those which have come to my knowledge, is the trituration or pressing of the root in water; and I believe that the colouring matter of the root has not been hitherto considered as so nearly insoluble in water, as I have reason to think it is.

Directions for establishing a manufactory of madder colour.

It were much to be wished, that in the present advanced state of Chemistry, some skilful analyser would investigate the properties of this very useful root; in which perhaps it will be found, that there are three, if not four, different colouring substances. Such are the processes and views, which I have thought it not improper to submit to the consideration of the Society of Arts, &c.

We want a good analysis of madder.

I have only now to describe the specimens which accompany this paper; assuring the Society, that they have been all prepared by my own hands entirely, and that I am therefore responsible

The specimens exhibited to the Society.

responsible for their having been produced by the processes stated, without the addition of any foreign matter whatever, excepting the cake ground up with gum, and the bladder of oil-colour, which were prepared from the colour which I gave him, by Mr. Newman, of Soho-Square, whose skill and fidelity are too well known to need any testimony in their favour.

It may be proper to add, that all the colours produced from the Dutch madder were prepared from the same parcel of crop madder, in order that the differences in them might proceed from the processes, and not from a variation in the qualities of the root, which, in different specimens, will produce different shades of colour under the same mode of treatment.*

1. Dutch madder, treated by process 1st.
2. Ditto - - - - process 2d.
3. Ditto - - - - process 3d.
4. Ditto - - - - process 4th.
5. Dutch madder, two ounces; alum, half an ounce; treated by process 2.
6. Dutch madder, two ounces; alum, one ounce; fermented two days, and then treated by process 2.
7. Produce of process 1, ground in gum by Mr. Newman.
8. Produce of process 1, ground in oil by Mr. Newman.

S—1. Smyrna madder, by process 1.

S—2. Ditto - - - process 2.

S—3. Ditto - - - process 3.

S—4. Ditto - - - process 4.

* Certificates accompanied the foregoing description, from Mr. Cotman and Mr. Munn, testifying the merits of Sir H. Englefield's madder lakes, as water colours; and also, from Messrs. West, Trumbull, Opie, Turner, Daniel, and Hoppner, speaking greatly in its favour, where it has been tried in oil-colours.

XIV.

*The Dutch Method of curing Herrings, extracted and translated from the German of Krünitz's Economical Encyclopædia (Oeconomische Encyclopædie), Article Haring, by J. HINCKLEY, Esq. F. S. A. **

THE vessels employed in this fishery, commonly called her-
ring-busses, from the Dutch name, are generally between 48 ^{of catching and} and 60 tons burthen, though some from 40 to 80 and 100 tons ^{curing of her-} are used. The largest of all carry 120 tons, are three-masted vessels, with one deck, and a cabin at each end; that a-head serving as a kitchen. Of the larger, the crews are 24 men, those of the smaller 18. They carry a few small guns and musquetry.

Their nets are dipped, or cast out, in the evening, and drawn up in the morning. It requires three hours to wind them on board. From the net, the fish are immediately put into baskets, while others of the crew are occupied till evening in gutting, salting, and packing. But although from ten to fifteen last are sometimes taken at a draught, the twelve persons usually employed for the purpose, cannot complete more than five last in a day.

During the three first weeks, from the 25th of June to the 16th of July, all the fresh-caught herrings are thrown into casks without picking, and conveyed to Holland, in the jagers, or yachts †, that accompany the herring-buss. But, after this period, immediately on being got on board and gutted, they are assorted into three qualities,—maiden her-

* From the Transactions of the Society of Arts, who have published it for the information of our fishers in North Britain, who appear to be well acquainted with the treatment from catching and landing, but not with the subsequent processes. Mr. Walter Baine, of Greenock, had the Society's silver medal for this object last season.

† These are small fast-sailing vessels, which follow the herring-busses, supply them with provisions, casks, salt, and other necessaries, and carry the fish that have been taken to the nearest port, where they are re-packed, and prepared for sending to the places of their destination.

rings,

DUTCH METHOD OF CURING HERRINGS.

Dutch method
of catching and
curing of her-
rings.

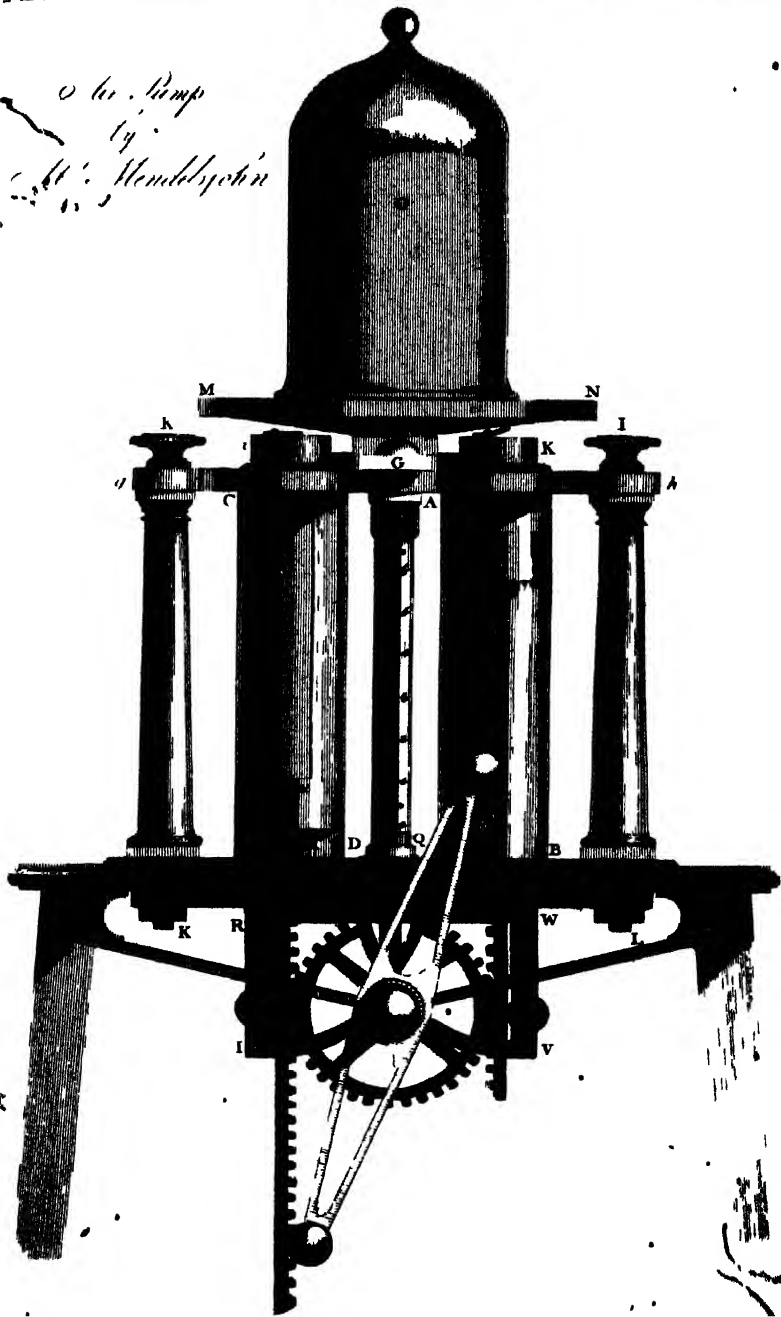
rings, full herrings, and shot herrings. The first of these are those taken earliest, and without row or melt, but which, though well flavoured, do not keep. Full herrings are those taken at Midsummer, on the point of spawning; from which the brand-herrings, so called from the barrels being marked with a hot iron, only differ in being caught later, re-packed immediately on arrival, and so close and hard pressed down, that they do not require re-packing at other places, but only new pickle; and are immediately expedited, or may remain on hand: whereas the other two sorts, not being so closely laid, must absolutely be re-packed. Shot-herrings are those which have spawned, or are taken in the act of spawning, in consequence of which they are thin and lean.

With the last two sorts the buffes themselves return (as soon as they have got their loading, or find no more fish), one after another, to port, where all three sorts, except the brand-herrings, before being expedited, are opened, salted anew, re-packed, and so heaped up, that fourteen casks are re-packed in twelve, which make a last. By a regulation of the States-General, this re-packing must be performed in the open air, where strict watch is kept, that the spoiling fish be carefully separated from the good, and the latter properly laid in the barrels, and strongly pressed down.

The Dutch fishery continues generally from twenty to twenty-six weeks, or even somewhat longer, namely, from the 25th of June to the middle of January. The Dutch fish only on the Scotch and English coasts, off Hittland, Fairhill, and Bocken, from Midsummer till the 25th of July; off Bocken or Serenial, from thence till the 14th of September; and in deep water, East of Yarmouth, and as far as the mouth of the Thames, from thence to the 25th of November, when the regular fishery ceases. But herrings are found not far from Yarmouth till the end of January, after which the fishery is prohibited, as the spawning season then commences.

(To be continued.)

o to Pump
by
St. Mendebychin



*W. Gough's
Theory of the
Speaking Trumpet.*

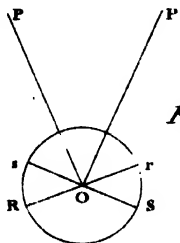


Fig. 4.

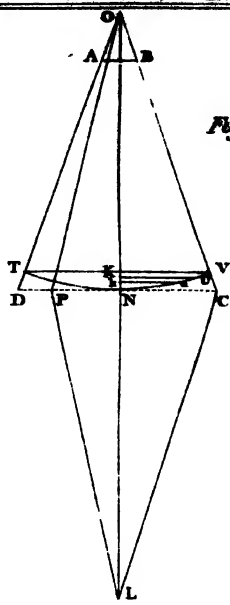


Fig. 3.

Part of the Air Pump.

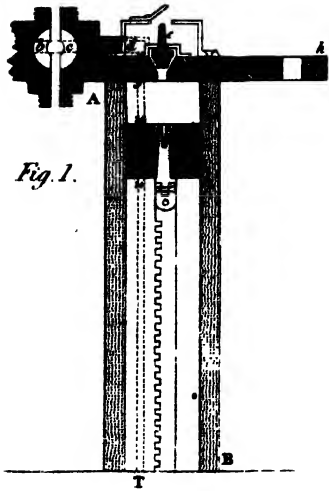


Fig. 1.

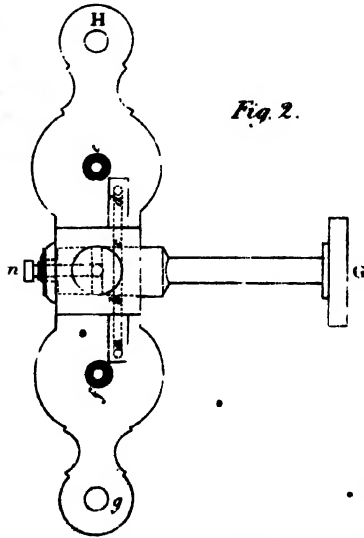


Fig. 2.

Fig. 1.
Improvement
in Weighing
Apparatus.

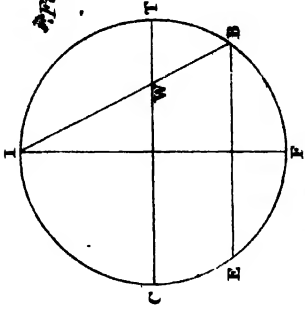
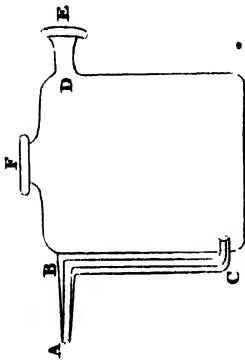


Fig. 3.

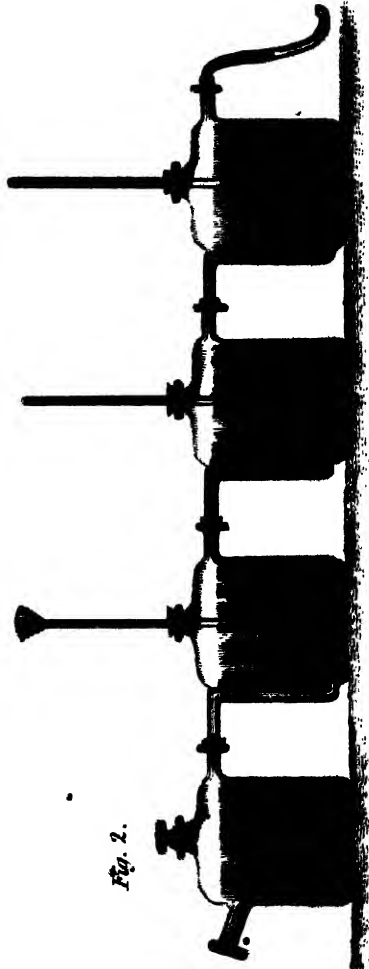


Fig. 2.

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1805.

ARTICLE I.

*A short Account of the Cause of the Disease in Corn, called by Farmers, the Blight, the Mildew and the Rust. Reprinted, and the Plates copied, with Permission, from a Memoir communicated by the Right Hon. SIR JOSEPH BANKS, Bart. P. R. S. &c. &c. &c. With some additional Notes.**

BOTANISTS have long known that the Blight in Corn is occasioned by the growth of a minute parasitic fungus or mushroom on the leaves, stems, and glumes of the living plant.

Botanists have long known that the blight is caused by a minute parasitic fungus;

* Immediately after the title in the original is the following prefatory note:

"The following brief Publication, suggested by the alarming state of the harvest in August last, would have been distributed before the end of wheat seed-time, had the engraver fulfilled his engagement.

"This circumstance will, it is hoped, be considered as a sufficient apology for the want of actual observations on the origin and progress of the disease. These it is presumed will be abundantly supplied in the course of the present year, by those intelligent agriculturists, whose residence in the country enables them daily to examine, not only the progress of their crops, but the origin and advances also of all those obstacles which nature has opposed to the success of agricultural labours, as if to awaken the energy

Felice Fontana published in the year 1767 an elaborate account of this mischievous weed,* with microscopic figures, which give a tolerable idea of its form; more modern botanists† have given figures both of corn and of grass affected by it, but have not used high magnifying powers in their researches.

but agriculturists have paid little attention to this fact.

Agriculturists do not appear to have paid, on this head, sufficient attention to the discoveries of their fellow-labourers in the field of nature; for though scarce any English writer of note on the subject of rural economy has failed to state his opinion of the origin of this evil, no one of them has yet attributed it to the real cause, unless Mr. Kirby's excellent papers on some diseases of corn, published in the Transactions of the Linnæan Society, are considered as agricultural essays.

Engravings of this destructive plant from accurate drawings by Mr. Bauer are here offered to their consideration.

On this account it has been deemed expedient to offer to the consideration of farmers, engravings of this destructive plant, made from the drawings of the accurate and ingenious Mr. Bauer, Botanical Painter to his Majesty, accompanied with his explanation, from whence it is presumed an attentive reader will be able to form a correct idea of the facts intended to be represented, and a just opinion whether or not they are, as is presumed to be the case, correct and satisfactory.

Organized structure of the surface of straw. Its pores or mouths, shut in dry and open in wet weather.

In order, however, to render Mr. Bauer's explanation more easy to be understood, it is necessary to premise, that the striped appearance of the surface of a straw which may be seen with a common magnifying glass, is caused by alternate longitudinal partitions of the bark, the one imperforate, and the other furnished with one or two rows of pores or mouths, shut in dry, open in wet weather, and well calculated to imbibe fluid whenever the straw is damp. ‡

By

"gies of reason, and to reward the farmer for the exertions of his intellectual faculties, by the satisfaction of surmounting them."

"Jan. 30th, 1805."

"JOS. BANKS."

* Osservazioni sopra la Ruggine del Grano. Lucca, 1767, 8vo.

† Cowerby's English Fungi, Vol. II. Tab. 140, Wheat Tab. 139. *Poa aquatica*.

The pores mouths on the surface of plants are designed to absorb moisture.

‡ Pores or mouths similar to these are placed by nature on the surface of the leaves, branches, and stems, of all perfect plants, a provision intended no doubt to compensate, in some measure, the want of loco-motion in vegetables. A plant cannot when thirsty

By these pores, which exist also on the leaves and glumes, it is presumed that the seeds of the fungus gain admission, and at the bottom of the hollows to which they lead, (see Plate IX. Fig. 1, 2,) they germinate and push their minute roots, no doubt (though these have not yet been traced) into the cellular texture beyond the bark, where they draw their nourishment, by intercepting the sap that was intended by nature for the nutriment of the grain; the corn of course becomes shrivelled in proportion as the fungi are more or less numerous on the plant; and as the kernel only is abstracted from the grain, while the cortical part remains undiminished, the proportion of *flour to bran* in blighted corn, is always reduced in the same degree as the corn is made light. Some corn of this year's crop will not yield a stone of flour from a sack of wheat; and it is not impossible that in some cases the corn has been so completely robbed of its flour by the fungus, that if the proprietor should choose to incur the expense of thrashing and grinding it, bran would be the produce, with scarce an atom of flour for each grain.

The seeds of the fungus enter the pores, germinate in the cellular texture, intercept the sap, and thus deprive the grain of its nutriment, shrivelling it by the abstraction of the kernel.

The flour may thus be almost totally intercepted.

Every species of corn, properly so called, is subject to the blight; but it is observable that spring corn is less damaged by it than winter, and rye less than wheat, probably because it is ripe and cut down before the fungus has had time to increase in any large degree.—Tull says that “white corn or bearded wheat, which hath its straw like a rush full of pith, is less subject to blight than Lammas wheat, which ripens a week later.” See page 74. The spring wheat of Lincolnshire was not in the least shrivelled this year, though the straw was in some degree infected: the millers allowed that it was the best sample brought to market. Barley was in some places considerably spotted, but as the whole of the stem of that grain is naturally enveloped in the sheath or basis of the leaf, the fungus can in no case gain admittance to the straw; it is how-

All corn is liable to the blight; spring corn less than winter, and rye less than wheat.

Historical facts.

go to the brook and drink, but it can open innumerable orifices for the reception of every degree of moisture, which either falls in the shape of rain and of dew, or is separated from the mass of water always held in solution by the atmosphere; it seldom happens in the driest season, that the night does not afford some refreshment of this kind, to restore the moisture that has been exhausted by the heats of the preceding day.

ever to be observed that barley rises from the soil lighter this year than was expected from the appearance of the crop when gathered in.

Little information was obtained by enquiry respecting blight; but its cause being now explained, the remedy it is hoped will be discovered.

Though diligent enquiry was made during the last autumn, no information of importance relative to the origin or the progress of the blight could be obtained: this is not to be wondered at; for as no one of the persons applied to had any knowledge of the real cause of the malady, none of them could direct their curiosity in a proper channel. Now that its nature and cause have been explained, we may reasonably expect that a few years will produce an interesting collection of facts and observations, and we may hope that some progress will be made towards the very desirable attainment of either a preventive or a cure.

Progress of the infection.

It seems probable that the leaf is first infected in the spring, or early in the summer, before the corn shoots up into straw, and that the fungus is then of an orange colour;* after the straw has become yellow, the fungus assumes a deep chocolate brown: each individual is so small that every pore on a straw will produce from 20 to 40 fungi, as may be seen in the plates, and every one of these will no doubt produce at least 100 seeds; if then one of these seeds tillows out into the number of plants that appear at the bottom of a pore in *Plate IX. Fig. 1, 2.* how incalculably large must the increase be! A few diseased plants scattered over a field must very speedily infect a whole neighbourhood, for the seeds of fungi are not much heavier than air, as every one who has trod upon a ripe puff-ball must have observed by seeing the dust, among which is its seed, rise up and float on before him.

Increase of the fungus incalculably rapid;

and its periods of generation very quick.

How long it is before this fungus arrives at puberty, and scatters its seeds in the wind, can only be guessed at by the analogy of others; probably the period of a generation is short, possibly not more than a week in a hot season: if so, how frequently in the latter end of the summer must the air be loaded as it were with this animated dust, ready, whenever a

* The Abbé Taffier, in his *Traité des Maladies des Grains*, tells us that, in France, this disease first shews itself in minute spots of a dirty white colour on the leaves and stems, which spots extend themselves by degrees, and in time change to yellow, and throw off a dry orange coloured powder. pp. 201, 340. *Note of Sir J. B.*

gentle breeze, accompanied with humidity, shall give the signal to intrude itself into the pores of thousands of acres of corn. Providence, however, careful of the creatures it has created, ^{provision in nature to check the increase of small plants and animals; the larger would not speedily be destroyed by} has benevolently provided against the too extensive multiplication of any species of being; was it otherwise, the minute plants and animals, enemies against which man has the fewest means of defence, would increase to an inordinate extent; this however, can in no case happen, unless many predisposing causes afford their combined assistance. But for this wise and beneficent provision, the plague of slugs, the plague of mice, the plagues of grubs, wire-worms, chafers, and many other creatures whose power of multiplying is countless as the sands of the sea, would, long before this time, have driven mankind, and all the larger animals, from the face of the earth.

Though all old persons who have concerned themselves in agriculture remember the blight in corn many years, yet some have supposed that of late years it has materially increased; this however does not seem to be the case. Tull, in his *Horsehoeing Husbandry*, p. 74, tells us, that the year 1725 "was a year of blight the like of which was never before heard of, and" which he hopes may never happen again;" yet the average price of wheat in the year 1726, when the harvest of 1725 was at market, was only 36s. 4d. and the average of the five years of which it makes the first, 37s. 7d.—1797 was also a year of great blight; the price of wheat in 1798 was 49s. 1d. and the average of the five years, from 1795 to 1799, 63s. 5d.* ^{The blight does not appear to have increased of late years.}

The climate of the British Isles is not the only one that is liable to the blight in corn; it happens occasionally in every part of Europe, and probably in all countries where corn is grown. ^{The blight in corn probably happens in all countries.}

* The scarcity of the year 1801 was in part occasioned by a mildew which, in many places, attacked the plants of wheat on the S.E. side only: but it was principally owing to the very wet harvest of 1800. The deficiency of wheat, at that harvest, was found, on a very accurate calculation, somewhat to exceed one fourth. But wheat was not the only grain that failed: all others, and potatoes also were materially deficient. This year the wheat is probably somewhat more damaged than it was in 1800, and barley somewhat less than an average crop. Every other article of agricultural food is abundant, and potatoes one of the largest crops that has been known. But for these blessings on the labour of man, wheat must before this time have reached an exorbitant price.—*Sir J. B.*

Italy is very subject to it, and the last harvest of Sicily has been materially hurt by it. Specimens received from the colony of New South Wales shew that considerable mischief was done to the wheat crop there in the year 1803 by a parasitic plant, very similar to the English one.

Common opinion that wheat in the neighbourhood of barberries is most frequently blighted.

It has been long admitted by farmers, though scarcely credited by botanists, that wheat in the neighbourhood of a barberry bush seldom escapes the blight. The village of Rollesby in Norfolk, where barberries abound, and wheat seldom succeeds, is called by the opprobrious appellation of Mildew Rollesby. Some observing men have of late attributed this very perplexing effect to the farina of the flowers of the barberry, which is in truth yellow, and resembles in some degree the appearance of the rust, or what is presumed to be the blight in its early state.

The barberry leaf is very subject to a mildew fungus;

It is, however, notorious to all botanical observers, that the leaves of the barberry are very subject to the attack of a yellow parasitic fungus, larger, but otherwise much resembling the rust in corn.

which may be transferred to corn.

Is it not more than possible that the parasitic fungus of the barberry and that of wheat are one and the same species, and that the seed transferred from the barberry to the corn is one cause of the disease. Mistletoe, the parasitic plant with which we are the best acquainted, delights most to grow on the apple and hawthorn, but it flourishes occasionally on trees widely differing in their nature from both of these: in the Home Park, at Windsor, mistletoe may be seen in abundance on the lime trees planted there in avenues. If this conjecture is founded, another year will not pass without its being confirmed by the observations of inquisitive and sagacious farmers.

Conjectures directed to remedy.

It would be presumptuous to offer any remedy for a malady, the progress of which is so little understood; conjectures, however, founded on the origin here assigned to it, may be hazarded without offence.

Probable fact as to its earliest appearance.

It is believed * to begin early in the spring, and first to appear on the leaves of wheat in the form of rust, or orange-coloured powder; at this season, the fungus will, in all pro-

* This, though believed, is not dogmatically asserted, because Fontana, the best writer on the subject, asserts that the yellow and the dark-coloured blight are different species of fungi.

ability, require as many weeks for its progress from infancy to puberty as it does days during the heats of autumn; but a very few plants of wheat, thus infected, are quite sufficient if the fungus is permitted to ripen its seed, to spread the malady over a field, or indeed over a whole parish.

The chocolate-coloured blight is little observed till the corn is approaching very nearly to ripeness; it appears then in the field in spots, which increase very rapidly in size, and are in calm weather somewhat circular, as if the disease took its origin from a central position.

May it not happen then, that the fungus is brought into the field in a few stalks of infected straw uncorrupted among the mass of dung laid in the ground at the time of sowing? it must be confessed, however, that the clover lays, on which no dung from the yard was used, were as much infected last autumn as the manured crops. The immense multiplication of the disease in the last season, seems however to account for this; as the air was no doubt frequently charged with seed for miles together, and deposited it indiscriminately on all sorts of crops.

It cannot however be an expensive precaution to search diligently in spring for young plants of wheat infected with the disease, and carefully to extirpate them, as well as all grasses, for several are subject to this or a similar malady, which have the appearance of orange-coloured or of black stripes on their leaves, or on their straw; and if experience shall prove that uncorrupted straw can carry the disease with it into the field, it will cost the farmer but little precaution to prevent any mixture of fresh straw from being carried out with his rotten dung to the wheat field.

In a year like the present, that offers so fair an opportunity, it will be useful to observe attentively whether cattle in the yard thrive better or worse on blighted than on healthy straw. That blighted straw, retaining on it the fungi that have robbed the corn of its flour, has in it more nutritious matter than clean straw which has yielded a crop of plump grain, cannot be doubted; the question is, whether this nutriment in the form of fungi does, or can be made to agree as well with the stomachs of the animals that consume it, as it would do in that of straw and corn.

Though the seeds of blighted corn afford little flour, they are each as good for sowing as the plumpest samples;

and are much more numerous in an equal measure.

The flour in the shrivelled grain is amply sufficient to nourish the minute plants till they take root.

It is a wasteful practice to take the plumpest grain for seed. The smallest though unfit for the mill are equally good for this use.

Arrangement of the grains in an ear of wheat. The lower ones supply the most food, but the upper are not inferior as seed.

It cannot be improper in this place to remark, that, although the seeds of wheat are rendered, by the exhausting power of the fungus, so lean and shrivelled that scarce any flour fit for the manufacture of bread can be obtained by grinding them, these very seeds will, except, perhaps, in the very worst cases *, answer the purpose of seed corn as well as the fairest and plumpest sample that can be obtained, and, in some respects better; for as a bushel of much blighted corn will contain one-third at least more grains in number than a bushel of plump corn, three bushels of such corn will go as far in sowing land, as four bushels of large grain.

The use of the flour of corn in furthering the process of vegetation, is to nourish the minute plant from the time of its developement till its roots are able to attract food from the manured earth; for this purpose one-tenth of the contents of a grain of good wheat is more than sufficient. The quantity of flour in wheat has been increased by culture and management calculated to improve its qualities for the benefit of mankind, in the same proportion as the pulp of apples and pears has been increased by the same means, above what is found on the wildings and crabs in the hedges.

It is customary to set aside or to purchase for seed corn, the boldest and plumpest samples that can be obtained; that is, those that contain the most flour; but this is unnecessary waste of human subsistence; the smallest grains, such as are sifted out before the wheat is carried to market, and either consumed in the farmer's family, or given to his poultry, will be found by experience to answer the purpose of propagating the sort from whence they sprung, as effectually as the largest.

Every ear of wheat is composed of a number of cups placed alternately on each side of the straw; the lower ones contain, according to circumstances, three or four grains, nearly equal in size, but towards the top of the ear, where the quantity of nutriment is diminished by the more ample supply of those cups that are nearest the root, the third or fourth grain in a cup is frequently defrauded of its proportion, and becomes

* 80 grains of the most blighted wheat of the last year, that could be obtained, were sown in pots in the hot-house; of these, seventy-two produced healthy plants, a loss of 10 per cent. only.

shrivelled

shrivelled and small. These small grains, which are rejected by the miller, because they do not contain flour enough for his purpose, have nevertheless an ample abundance for all purposes of vegetation, and as fully partake of the sap, (or blood, as we should call it in animals,) of the kind which produced them, as the fairest and fullest grain that can be obtained from the bottoms of the lower cups by the wasteful process of beating the sheaves.

EXPLANATION OF THE PLATES.

Plate IX.

Fig. 1. A piece of the infected wheat straw—natural size: at *a* the leaf sheath is broken and removed, to shew the straw which is not infected under it.

Fig. 2. A highly magnified representation of the parasitic plant which infects the wheat: *a* in a young state; *b* full grown; *c* are two plants bursting and shedding their seeds when under water in the microscope; *d* two plants burst in a dry state; *e* seems to be abortive; *f* seeds in a dry state; *g* a small part of the bottom of a pore with some of the parasitic fungi growing upon it.

Fig. 3. A part of the straw of *fig. 1*, magnified.

Fig. 4. Part of *fig. 3* at *a b* more magnified.

Fig. 5. Part of a straw similar to *fig. 3*, but in its green state, and before the parasitic plant is quite ripe.

Fig. 6. A small part of the same, more magnified.

Plate X.

Fig. 1. A highly magnified transverse cutting of the straw, corresponding with *Fig. 4*, Plate I. shewing the insertion of the parasite in the bark of the straw.

Fig. 2. A longitudinal cutting of the same; magnified to the same degree.

Fig. 3. A small piece of the epidermis of a straw, shewing the large pores which receive the seed of the parasite; the smaller spots observable on the epidermis, are the bases of hairs

hairs that grow on the plant of the wheat whilst young, but which fall off when it ripens, magnified to the same degree as the preceding figures.

II.

Theorems respecting the Properties of the Sides of Triangles intersected by Right Lines drawn from the three Angles so as to meet in one Point. By Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

Middlesex, March 1, 1803.

CERTAIN PROPERTIES OF TRIANGLES. GEOMETRICIANS have paid but little attention to the properties of the sides of triangles, which are intersected by right lines, drawn from the three opposite angles so as to meet in one point. Perhaps all the theorems of this kind that have been hitherto published, are contained in the second book of Emerson's Geometry, with the single exception of a proposition given by Mr. Landen; who has proved that if one of the internal angles of a triangle, and the two opposite external angles of the same be bisected, the right lines, drawn for this purpose, will also meet in a common point. The following propositions take a more general view of the subject; and as some mathematical papers have appeared of late in the Philosophical Journal, I have ventured to ask a place for the present sheet, in your valuable miscellany.

I remain, &c.

JOHN GOUGH.

P. S. In vol. X. page 66, line 2, for $5\frac{1}{2}$ yards read $3\frac{1}{2}$ yards.

Proposition 1st. Let three right lines CM, BN and AS, (Plate XI. Fig. 1, 2.) drawn from the three angles of a triangle ABC, intersect the opposite sides, produced or not, in the points M, N and S, and also meet each other in a common point O; then if all the points M, N and S lay betwixt the

the angular points B, A and C, the place of O will be within the triangle, and the three lines CO, BO and AO will fall wholly within the same; but if one of these points, as S, lay in BE, which is CB produced, the place of O will be external to the triangle; in which case, one of the two remaining points of intersection will lay in its respective side produced; but the situation of the other will always be betwixt the angular points of the third side.

Demonstration.

Case 1st. Let M, N and S (*Fig. 1.*) be betwixt the angular points B, A and C, in which case, the triangles ABC, MBC are of the same altitude; consequently they are as their bases AB, BM, *Euc. 1.6*; but BM is a part of AB by hypothesis; therefore the triangle MBC is a part of ABC, (*Simpson's Euclid. D. 5.*) that is, the triangle ABC contains the triangle MBC; for the same reason, the same ABC contains the triangle SAB; but the figure MBSO is common to the triangles MBC, SAB; therefore it is contained in the triangle ABC; consequently the place of O is within the same: now as the points A, B, C and O are all found in the space ABC, the lines AO, BO and CO must also lay wholly within the same triangle.

Case 2. When the point S falls in BE or CB produced, (*Fig. 2.*) the angle CBA is external to the triangle ABS; therefore it is greater than the angle BSA, (*Euc. 17. 1.*) and the sum of the angles CAB, ACB is less than the angles CAS, ACS taken together, (*Euc. 32. 1.*); hence the angle CAB is less than CAS; consequently the right line AS, produced at pleasure, falls wholly without the triangle ABC. Now if the remaining two lines CM and BN be supposed to cut the sides BA and AC not produced, the place of O, their common intersection, will be in the space ABC, by *Case 1*: but this is impossible; for O is in the right line AS by hypothesis; which has been shewn to fall wholly without the triangle ABC, consequently one at least of the lines CM, BN must also lay without the same space; let this be BN, and draw OK parallel to CA; then the external angle OKE of the triangle OKC is greater than the internal and opposite angle

Corollary. Hence it appears that if two lines CM and BN be drawn, and their intersection O lay within the triangle ABC , the place of the third point S will be in the side BC unproduced. But if O lay without the triangle, the place of the third point will depend on the situation of the given points; namely if these be in two sides produced, the place sought will lay in the remaining side unproduced; but if either of the given points fall between two angles of the triangle, the third will be found in the remaining side produced.

Proposition 2. If a right line CM (*Fig. 1.*) be drawn from C , one of the angles of a triangle ABC , to cut the opposite side AB , produced or not, in M , and if from the point M , perpendiculars, MF , MH be let fall upon the sides AC , CB , the rectangle BM , MF will be to the rectangle AM , MH , as BC is to CA .

Demonstration. Draw AP , BQ perpendicular to BC , CA ; then we have the following proportions, by similar triangles; as $MB : MH :: AB : AP$; and as $MF : MA :: BQ : BA$; hence as $MB \times MF : AM \times MH :: BQ : AP$. But as $BQ : AP :: BC : CA$, *Euc. 11.6*; therefore the rectangle MB , MF is to the rectangle MA , MH as BC is to CA . *Euc. 11.5. Q. E. D.*

Cor. 1st. If Oh , Of be drawn perpendicular to BC , CA , from any point O in MC ; the rectangle MB , Of will be to MA , Oh as BC is to CA ; for by similar triangles as $fO : Oh :: FM : MH$; hence as $MB \times Of : MA \times Oh :: MB \times MF : MA \times MH$; consequently as $MB \times Of : MA \times Oh :: BC : CA$. *Euc. 11.5.*

Cor. 2nd. Let the triangle ABC be isosceles, having the sides BA , AC equal; also let CM , BN , drawn to the opposite sides BA , AC meet in O ; then the point S is given, in which AO , produced if necessary, cuts the remaining side CB . For draw Of , Og perpendicular to CA , AB , and we shall have, as $BS \times Of : CS \times Og :: BA : AC$, *Cqr. 1*; but $BA = AC$,
 $BA,$

B A, A C are equal by hypothesis; therefore $BS \times Of = CS \times Og$; hence as $Og : Of :: BS : SC$; consequently the point **S** may be found, proper attention being paid to *Cor. Prop. 1.* Certain properties of triangles.

Proposition 3. If three right lines **CM, BN, AS**, drawn from the three angles of a triangle **ABC** (*Fig. 1 and 2*) meet each other in a point **O**, and the opposite sides **BA, AC** and **CB**, in **M, N** and **S**; the segments of any one side are as the rectangle under the alternate segments of the remaining two sides; *i. e.* **BS** is to **SC** as the rectangle **BM, AN** to the rectangle **CN, AM**.

Demonstration. Draw **Of, Og, Oh** perpendicular to **CA, AB** and **BC**. Then as $BC : CA :: BM \times Of : AM \times Oh$; *Cor. 1. Prop. 2*; for the same reason as $AB : BC :: NA \times Oh : NC \times Og$; hence as $BA : AC :: MB \times NA \times Of : NC \times MA \times Og$; but as $BA : AC :: BS \times Of : CS \times Og$; consequently **BS** is to **SC** as the rectangle **BM, AN** is to the rectangle **CN, AM**. *Q. E. D.*

Cor. If two right lines **CM, BN** be drawn from two angles of any plain triangle **ABC**, so as to meet each other in a point **O**, and the opposite sides **BA, AC** in **M** and **N**; a point **S** may be found in the remaining side, produced if necessary, so that if **SA** be joined, the right line **AS** shall pass through **O**. For since the right lines **BM, MA, AN** and **NC** are given, the rectangles **BM, AN** and **CN, AM** are also given; consequently the ratio of these rectangles is given: but the right line **AO** cuts the side **CB**, produced or not, according to circumstances, in the same ratio, by the last proposition; therefore if both **M** and **N** lay betwixt the angular points of the triangle, or in their respective sides produced, divide **BC** in **S**, so that **BS** may be to **SC** in the ratio of the rectangles **BM, AN** and **CN, AM**; then will **S** be the required point by *Cor. to Prop. 1*; but if **M** be situated betwixt the angles **B, A**, and **N** lay in **CA** produced; find **S** in **CB** also produced, so that **BS** may be to **SC** as the rectangle **BM, AN** is to the rectangle **CN, AM**; and **S** will be the required point, by the corollary last referred to.

Prop. 4. Let three right lines **CM, BN** and **AS** (*Fig. 1 and 2*) drawn from the three angles of a triangle **ABC**, intersect each other in a common point **O**, and meet the opposite

Certain prop-
ties of triangles.

Let the right lines BA , AC and CB , in the points M , N and S ; if AN be to NC as APB is to B^pC ; but BM be to MA as B^pC is to C^pA ; then will BS be to SC as the product of B^pA into B^pC is to the product of B^pC into A^pM .

Demonstration. As $AN : NC :: B^pA : B^pC$; and as $BM : MA :: B^pC : C^pA$ by hypothesis; hence as $BM \times AN : CN \times AM :: B^pA \times B^pC : B^pC \times C^pA$; but as $BM \times AN : NC \times AM :: BS : SC$; consequently BS is to SC as the product of B^pA into B^pC is to the product of B^pC into C^pA .
Euc. 11. 5. Q. E. D.

Corollary 1st. The preceding theorem is general; for the ratios of BM to MA , and of AN to NC can be expressed by some power or powers of the lines BC , CA and AB , BC , which contain the angles opposite to the sides BA and AC ; therefore if the points M , N and S be arranged according to the corollary to Proposition 1st, and the segments betwixt these points and the adjacent angles have the ratios assigned in the present theorem, right lines, drawn from M , N and S to the opposite angles, will all meet in one point.

Cor. 2. When r and p are equal, BS is to SC as B^pA is to A^pC by the proposition; therefore if the segments betwixt the points M , N , S and the adjacent angles are in the ratios of the adjacent sides raised to any given power, right lines, drawn from M , N and S to the opposite angles, will all meet in one point by *Cor. 1st*.

Cor. 3rd. Let $p = 0$; then as $BS : SC :: B^0A : A^0C$ by the proposition; but $B^0A : A^0C$ is the ratio of equality; therefore if M , N and S bisect the three sides of a triangle, lines drawn from them to the opposite angles will meet in one point by *Cor. 2nd*.

Cor. 4. Let $p = 1$; also let M and N intersect the sides BA , AC not produced; then S lays between the angular points B , C , *Cor. Prop. 1*; and as $BS : SC :: BA : AC$, *Cor. 2*; therefore the right line SA bisects the angle BAC ; consequently if three right lines bisect the three internal angles of a triangle, they shall all meet in one point by the last proposition.

Cor. 5. Let $p = 1$ as before; also suppose M to be in the side BA not produced; but let N be without the triangle; then S is in CB produced, *Cor. Prop. 1*; but as $BS : SC ::$

$BA : AC$, *Cor.* 2; therefore SA bisects the angle BAN . (*Simpson's Euclid.* .6 A); also as $BM : MA :: BC : CA$; therefore CM bisects the angle ACB , *Euc.* 3.6; consequently if two lines bisect any two of the external angles of triangles, and a third line bisect the internal angle opposite to these angles, these lines shall meet in one point by the last proposition.

Prop. 5. If BM be to MA as B^pC to C^pA ; and hO , Of be perpendicular to BC and CA ; hO is to Of as $B^{p-1}C$ to $C^{p-1}A$.

Demonstration. As $BM \times Of : AM \times Oh :: BC : CA$, *Cor.* 1. *Prop.* 2; and as $B^pC : A^pC :: BM : MA$ by hypothesis; therefore as $B^pC \times Of : A^pC \times Oh :: BC : CA$; consequently as $Oh : Of :: B^{p-1}C : C^{p-1}A$. Q. E. D.

III.

A Communication on the Use of Green Vitriol, or Sulphate of Iron, as a Manure; and on the Efficacy of paring and burning depending, partly, on Oxide of Iron. By GEORGE PEARSON, M. D. Honorary Member of the Board of Agriculture, F. R. S. From a Communication made by him to the Board, and inserted in the fourth Volume of their Transactions.

(Concluded from page 214.)

APPENDIX.

THE following facts, lately discovered by most respectable chemists, appear to be worth adding to the preceding memoir, as they serve to shew that other salts, besides sulphate of iron and certain earths, may be employed advantageously as manures, although like iron they have been esteemed deleterious to plants.

1. *Ashes of Pit Coal are a good Manure for Grass.*

My much valued friend, the Rev. William Gregor, of The ashes of pit coal are a good manure for grass.
 Grampound, on examination of the ashes of coal from Liverpool, found them to contain both sulphate of magnesia and sulphate of lime, especially the former, salt. I apprehend that these ashes also contain oxide of iron, or perhaps sulphate

USE OF GREEN VITRIOL

of iron. These ashes, says Mr. Gregor, "*skeaded*" * over grass apparently produced good effects notwithstanding the sulphate of magnesia, which I was well assured they contained. See Nicholson's Journal, Vol. V. p. 225.

Sulphate of magnesia may not be hurtful to plants, tho' pure magnesia is.

From this observation of Mr. Gregor, it seems he is aware of the prevailing popular opinion, that sulphate of magnesia is not favourable to vegetation; and to reconcile his fact with the unfriendly nature of magnesia to plants, as discovered by Mr. Tennant, he observes that the effects of sulphate of magnesia may be very different from those of magnesia and carbonate of magnesia. I apprehend it is the magnesia (cal-cined magnesia) only which this learned chemist found hurtful to vegetables, as the discovery was made on the examination of Nottingly lime, which the farmers near Doncaster employ as a manure, while they reject the lime of their own neighbourhood. In the latter Mr. Tennant met with magnesia, and in the former none. See the account of this important discovery in the Philos. Transactions.

2. *The Earth from Ashes called Cinis, is a durable and efficacious Manure; by Professor Mitchill, of New York, one of the Representatives in Congress. Addressed to Dr. Pearson.*

Earth from ashes is a good manure.

Dr. Mitchill, in a letter addressed to me on cinis, or earth found in the ashes of wood, has made some observations relative to the preceding memoir, which seem worthy of notice.

"Ashes of wood contain very commonly sulphate of potash, also phosphoric acid, besides other well known salts; but after these salts are separated by lixiviation, there remains a *peculiar earth*, and a small proportion of iron. This earth differs from lime, baryt, magnesia, strontian, or any other known species of earth. I would call it *cinis*, for plentiful, common, and important as it is, science has not dignified it with a name. To judge of the excellence of this earth as a manure, after all the salts are extracted from soap boilers' ashes, the earth sells for ten cents the bushel, and notwithstanding this high price, it is not unusual for the farmer to pay for the article twelve months beforehand. When ploughed into sterile ground, at twelve loads per acre, it produces great crops of wheat, clover, and other sorts of grass and grain, and its fertilizing operation will last twenty years. Although some

* From *Extrada*.

Of the other ingredients of the ashes left after lixiviation may prove beneficial, yet the effects are chiefly from the cinis, or new named earth.

"This earth, which is so prized in America as a manure, was esteemed of old in Asia, as an ingredient in a cement: among the ancient Syrians, it was one of the materials forming the plaster of their walls; and as it holds an intermediate place between lime and potash, it can easily be conceived how it may act both as a cement and a manure. It is to be hoped, chemists will turn their attention to this important subject." See Tilloch's *Philos. Magazine*, Vol. VII. p. 273, for the whole of this interesting letter.

3. *Several Metallic Salts promote Vegetation, shewn by the Experiments of Professor Barton, of Philadelphia.*

Letter from Benjamin Smith Barton, M. D. Professor of Medicine in the College of Philadelphia, to Dr. Pearson, containing Experiments with Metallic Solutions to determine their Effects on Plants.

SIR,

Philadelphia, Oct. 28, 1802.

IN the *Annals of Medicine* for the year 1801, you inform us, that you have lately read a paper at the Board of Agriculture, "containing an account of the effects of a saline body collected from peat, as a most powerful manure, which turns out to be sulphate of iron; a substance (you remark) hitherto considered to be a poison to plants." This piece of intelligence gave me much satisfaction. I have, for some years, been engaged in an extensive series of experiments, relative to the effects of various stimulating articles, such as camphire, &c. upon vegetables; and on the absorption of certain powerful mineral substances into the organic system of vegetables. In numerous instances I have subjected the stems and leaves of plants, young and old, large and small, to the influence of the sulphates of iron and copper. I have found, that both of these metallic salts are very greedily absorbed by vegetables, inasmuch that I have detected the presence of iron in the vessels of a branch of mulberry, at the height of five or six feet above the place of immersion in a solution of the sulphate of this metal. A full account of my experiments, I design

Direct experiments on the effect of metallic salts on vegetation.

to communicate to the public in two memoirs. Permit me to observe, in the meanwhile, that the sulphate of iron, applied to vegetables in the manner I have mentioned, "is only (to use your own words) a poison, like almost every thing else, from the over-dose." *In several of my experiments, the branches of vegetables that were placed in vessels containing solutions of sulphate of iron and copper, lived longer and exhibited more signs of vigour, than similar branches that were placed in equal quantities of simple water.* It is true, that, in many other experiments, these metallic salts proved fatal to my plants; but this was when I employed too large a dose. In like manner I had found several years ago*, that camphire, by greatly stimulating, often kills vegetables; and yet, when properly dosed, this is a very wholesome stimulant to plants. I had also found, that large doses of nitre (which is unquestionably a powerful stimulant both with respect to animals and vegetables) produce an appearance like genuine gangrene in the leaves of vegetables: and yet it is certain that nitre, when it is judiciously dosed, may be made to greatly assist the healthy vegetation of plants.

Excuse the liberty I have taken in troubling you with these few loose hints, and permit me to subscribe myself,

Sir, your very humble and obedient Servant, &c.

BENJAMIN SMITH BARTON.

4. *Sulphate of Iron in the Peat of Russia, found by Professor Robison.*

Sulphate of iron
exists in Russian
peat.

Something else besides vegetable matter is necessary to form peat or black moss of the moors. The smell of burning peat is different from that of vegetable matter. Peat ashes, says the Professor, always contain a very great proportion of iron; he has been three places in Russia where there is superficial peat moss, and in all of them *the vitriol is so abundant as to effloresce*. In particular, on a moor near St. Petersburg, the clods shew the vitriol (sulphate of iron) every morning when the dew has evaporated. According to this learned Professor's observation, the sulphate of iron in pit coal may be accounted for in the following manner: "peat mosses form very regular

* See Transactions of the American Philosophical Society, Vol. IV. No. xxvii.

strata, lying indeed on the surface; but if any operation of nature should cover this with a deep load of other matter, it would be compressed and rendered very solid: and remaining for ages in that situation, might ripen into a substance very like pit coal. See the Medical and Chirurgical Review for November 1803.

5. *Mr. Anstey's Testimony of the Use of Peat Dust and Peat Ashes.*

SIR,

Houghton Regis, Dec. 3, 1801.

I received yours, dated the 18th of November last, in Testimony of which you requested me to inform you what experiment I had ^{Mr. Anstey:} made from the turf dust, taken from Tingrith Moor. I have made use of the ashes and dust near thirty years, and I frequently lay on from eighty to a hundred bushels per acre. Our land is dry and very thin stapled, owing to the chalk rock laying so very near the surface; it encourages vegetation in moist warm weather; but when hot and dry, the reverse. We never mix any other manure with it. It costs about four-pence per bushel, including all expences.

We chiefly spread it on our seed grass, clover, &c.

I am, Sir, your humble Servant,

JOS. ANSTEY.

IV.

On Spectacles. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

DEAR SIR,

Lynn, February 16, 1805.

IN a former paper, I pointed out a property in spectacles, ^{Position of spectacles,} which had been overlooked by the writers on optics; but as some of your readers entertain opinions contrary to mine, the subject seems to require further investigation.

What I have advanced concerning spectacles is, that we see better by oblique, than by direct pencils of rays *. ^{we viz. that we see better by oblique, than by direct rays.}

Philosophical Journal, Vol. VII. p. 291.

It has been demonstrated by mathematicians, that an oblique pencil of rays has its focus a little nearer the lens, than a direct pencil; but how much this difference amounts to, at different angles, seems not to have attracted their attention. This difference, however, is too considerable to be disregarded, when the rays of light fall very obliquely.

Considerations
relating to
oblique pencils.

For let LL (*Fig. 3. Pl. xi.*) represent a double convex lens, F its focus, AF its axis, and ab a ray parallel to it, which will be refracted to F , after passing through the lens. Also let cb and dd be two oblique rays falling upon the lens, at b and d .

Then the rays near AF falling almost perpendicularly upon the sides of the lens, are less bent than the rays cb , dd , which fall obliquely on both sides of the lens. "For the more oblique they are, the more they are bent, and turned out of their direct way. And consequently the oblique rays cb , dd , will sooner intersect in G , than those at F ." (*Emerson's Optics*, page 124.) When the angle abc is very considerable, the difference between $x F$, and $y G$, will likewise be very considerable. And the following short account of my experiments will throw some light upon the truth of this proposition.

Spectacles
obliquely placed.

In the spectacles that I use, the reading distance of oblique rays is four inches nearer the glasses, than the distance at which I can see best, with the direct rays.

And by experiments made with a lens of 24 inches focal distance, and 4 inches in diameter, contracted to $\frac{3}{4}$ of an inch, I found the focal distance of an oblique pencil of rays 8 inches shorter than the focal distance of the direct rays. The axis of the lens, when placed for the oblique rays, made an angle of 26° with a line drawn from its centre, to the candle with which the experiments were made. And the distance between the candle and the lens measured about 10 feet, when the lens was placed to receive the direct rays.

Cor. 1. When an object is seen through spectacles, by oblique rays, it appears larger than by direct rays.

For the object is seen under the greatest angle, by those rays that are most refracted.

Cor. 2. When an object is viewed by oblique rays through spectacles, it is seen more distinctly with one eye, than with the other.

For

For the rays fall more obliquely upon one of the glasses, than upon the other.

Cor. 3. The direct pencils of rays are but seldom used, by a person whose spectacles are of the longest focal distance, that will afford him distinct vision.

For he generally looks through those parts of his glasses, which are remote from their centres, in consequence of that motion which is given to the eyes, to view objects in different directions.

I am, Dear Sir,

Your most obedient Servant,

EZEKIEL WALKER.

V.

Remarks on certain Passages in Dr. Thomson's Chemistry, together with some Experiments on Sundarach and Mastic. In a Letter from Mr. R. MATTHEWS.

To Mr. NICHOLSON.

SIR,

IN a work of such importance as Dr. Thomson's Chemistry, which must be so frequently referred to by the lovers of that science, the slightest inaccuracy ought not to pass unnoticed: the following observations therefore, perhaps may be considered sufficiently interesting to deserve a place in your valuable Journal.

In his 4th Vol. page 307, Dr. Thomson has the following remark: "Hitherto it has been affirmed by all chemists both ancient and modern, that the alkalies do not exert any action on the resins." This remark I admit may be generally true; but although most chemists have been negligent in ascertaining the effects of alkalies upon substances of this nature, I conceive some of them were not unacquainted with the power they possess of dissolving resinous bodies.

Boerhaave, for instance, in his elements of chemistry, page 545, expressly says, that, "both the fixed and volatile alkalies have a dissolving power, upon animal, vegetable, and mineral substances, so far as these contain oils, balsams, gums, resins, or gummy resins, &c."

Hofmann.

and by Hoffmann. Hoffmann also, by the method he employed in dissolving amber with caustic nitre, evinced that he was acquainted with this property in alcalies. Dr. Thomson describes Mr. Hatchett as the first discoverer of the action of alcalies upon resin. The facts above quoted will lessen the originality, without diminishing the value of Mr. Hatchett's ingenious experiments.

How therefore it can be true that, "all chemists both ancient and modern, have affirmed, that the alcalies do not exert any action on the resins" I cannot easily discern, unless indeed we impute to them the most palpable contradictions, an error which I believe is not often found in their writings.

Dr. Thomson asserts, that one fifth of sandarach is insoluble in alcohol.

Page 340 in the same volume, Dr. Thomson asserts, that sandarach is not soluble in alcohol, about a fifth part of it remaining undissolved; and describes the insoluble part as possessing peculiar properties, differing from sandarach, and calls it *sandaracha*. It ought not to be concluded, that Dr. Thomson himself can have chemically examined every substance he must have occasion to describe in a work which embraces so great a variety; but the importance of sandarach as an ingredient in varnishes being nearly equal to that of any other resin, this substance certainly deserves more attention from chemists than has hitherto been bestowed on it. The properties of this resin as mentioned by Dr. Thomson, are stated to rest solely on the authority of Giese, no other chemist having examined it.

Experiment. It proved soluble in eight times its weight of alcohol.

Upon trial I found that sandarach was soluble in eight times its weight of alcohol, a very minute proportion only being left, which appeared to consist almost entirely of extraneous matter. Consequently the substance Dr. Thomson has denominated *sandarucha* can have no existence in this resin. It is proper to observe, that the resin I made use of was selected fine, and is known in commerce by the names of gum juniper and sandarach. From the obvious disagreement in the results, perhaps we ought to conclude, that the substance employed by Giese in his experiments was not true gum sandarach*.

* Looking to some chemical notes, I find that when three or four tears of sandarach were put into pure alcohol, and left for a day, a small quantity of thick fluid remained at the bottom, which, however, was taken up by agitation, and the whole became semiopaque:—It is remarkable, that this substance is not soluble in tallow or oil, as common resin is. N.

Speaking of mastich, vol. 3, p. 311. Dr. Thomson says, it is readily soluble in alcohol. In the same valuable volume, page 332, he informs us, that "that part of mastich which is insoluble in alcohol is said to be pure caoutchouc."

Dr. Thomson's account of mastich.

In investigating this apparent contradiction, I examined the action of alcohol upon mastich, and found that nearly a fifth part of it remained undissolved. This residuum, after repeated washings in alcohol, was white, considerably elastic, and adhesive; it was inflammable; and when heated became brown, emitting inflammable gas. In this state it had much the appearance of common Indian rubber, but somewhat glutinous: It was not in the least acted on by water.

The Author's experiments.

Alcohol leaves one fifth of mastich undissolved, resembling Indian rubber.

I was induced to try the effects of different menstrua on this residuum, and on the elastic gum caoutchouc, with a view to ascertain their identity or otherwise. The following are the results I obtained:

Comparative experiments on the residue of mastich, and on caoutchouc.

Sulphuric ether, previously washed with distilled water, dissolved this residuum as well as caoutchouc.

Alcohol precipitated both these substances from their solvent in the form of a white curd.

Water had no action on the solutions.

By the nitric acid the residuum was converted into a yellow brittle porous mass, nitrous gas being disengaged from the acid.

Nitric acid did not act so easily on caoutchouc; but when raised to the boiling point it changed it into a similar substance.

With the sulphuric acid a substance like charcoal was formed, the acid assuming a dark port wine colour, part of it being converted into sulphureous acid.

The same effect was produced by the sulphuric acid on caoutchouc, though not without the assistance of heat.

Neither the muriatic or oxymuriatic acids appeared to have any action on either of these substances.

Acetic acid dissolved a small portion of the residuum, but had no action on caoutchouc.

Solutions of potash and ammonia produced no effect on either the residuum or caoutchouc.

From the above experiments it will be observed, that the acids act with greater facility on the insoluble part of mastich, than on caoutchouc. Notwithstanding this difference in their action,

These appear to be the same.

action, I think it may be inferred, from the coincidence in their effects, that these bodies are similar substances. Probably the action of the acids on the residuum was assisted by a small portion of resin which it was combined with, and which it defended from the action of the alcohol employed to dissolve the mastic.

I am, your obedient Servant,

R. MATTHEWS,

March 13, 1805.

VI.

The Dutch Method of curing Herrings, extracted and translated from the German of Krünitz's Economical Encyclopædia (Oeconomische Encyclopædie), Article Hüring, by J. HINCKLEY, Esq. F. S. A.

(Concluded from p. 224.)

Dutch method
of catching and
curing herrings.

HERRINGS, however, are equally good, wherever they are taken, provided they be but caught in the proper season, and well managed. As they die immediately on quitting their element, salting and packing are the circumstances which principally affect their quality. The superior excellence and flavour of Dutch herrings, above those of all other countries, arises from the close attention and indefatigable industry employed. Every thing, however minute, both as to season and management, which can maintain the reputation they have enjoyed for more than two hundred years, is most punctually observed; and above thirty ordinances on the curing and management of herrings are recited by Sir William Temple, in the 17th century.

Herrings, cured with "Scotch salt, very quickly decay. Those of Norway are cured in the same manner as the Scotch, but with French salt, and packed in fir or deal; in consequence of which they are worse, and less palatable, as they leave a sour-taste in the mouth, and soon spoil. In like manner, other nations are equally careless in assorting the fish, and to this may be attributed the precedency which the Dutch herrings have so long maintained. The Dutch catch their fish regularly and early off Hittland, from the 25th of June, because

because they are then, and to the beginning of July, ^{Dutch method} ^{of catching and} ^{curing herrings,} ~~fastest~~ after which time, the nearer they approach the coast, the leaner and worse they are.

It redounds no less to the honour of the Dutch, than to their advantage, that they pay the utmost possible attention to these rules: according to which, the fish must be taken at the proper season, properly salted, well assorted, and rightly packed; to do all which, the captain and sailors are by several laws obliged to bind themselves by oath, before they sail. There are also overseers well paid, that they may not betray their trust, but watch and enforce every the minutest regulation; to which circumstance also, the pre-eminence of Dutch herrings throughout the world may partly be ascribed.

As soon as the herrings are taken out of the water, they are thrown either upon the end of the deck, which has been cleared, and made perfectly clean for the purpose, or into baskets; and then *gipped* (the gills and guts taken out) with a knife, by some of the crew, who are solely employed therein, having been brought up to that practice. The milt, or roe, however, is always left in the fish.* What are taken during one night, are, before the following sun-set, neatly and skilfully laid in oaken barrels, coarse Spanish or Portuguese bay salt being strowed between. This the fishermen of other countries either entirely neglect, or less carefully perform, being less scrupulously nice; because they either go out to sea later, or, like the Scotch, commence fishing too soon; or only navigate small boats near the coast; do not kill the fish with a knife, or gut, salt, or pack them, while on board, but throw them down in a boat, and when fully laden, go on shore, proceed at their leisure, cast the fish on the sea-coast in considerable heaps, where they are even suffered to lie sometimes several days, before they are gutted, salted, and packed, in consequence of which they grow stale and ferment. The Dutch, on the contrary, indefatigably pursue their method day and night, during twenty, twenty-four, or twenty-six weeks, be the weather what it may. Hence, their fish are usually more tender, better flavoured, and not so very salt as the English and the Scotch.

* Mr. McCulloch's Treatise (see note, page 413) says, they should also be kept cleanly, and out of the sun, as well as frost or rain,

There

Dutch method
of catching and
curing herrings.

There are two methods of salting and preserving herrings for a considerable length of time. The one is called white salting, the other red. The former is thus performed. Immediately on being taken, the fish are gutted, as above described, and washed in clean water. Then salt is sprinkled on them, either internally, or both within and without, and the fish, being thrown into large baskets with handles, are well roused, (or shaken about a few times) that the salt may the better diffuse itself, and penetrate: or, lastly, which is the best method, they are thrown into a tub filled with a strong brine made with bay salt and fresh water, in which an egg will swim. In this pickle they are left upon deck in the open air, provided the weather be good, during twelve or fifteen hours; but, if circumstances require, a good deal longer, and are well stirred (especially if pickled on shore) with shovels several times, that the salt may the better and more generally penetrate every where. Lastly, to pack them properly, they are taken out of the pickle, suffered to drain sufficiently, and then packed in barrels, which are strewed at bottom pretty thickly with salt, and, if there is time enough, they are neatly laid in strata or layers, always strewing sufficient salt upon each layer. But, if the abundance of fish be too great, they are thrown in promiscuously, with as much salt as is requisite to preserve them from spoiling. When this is done in strata, each new layer is pressed down hard upon the preceding. This last-mentioned process, however, cannot easily be attended to at sea, especially when the fishery is very abundant. Hence, if the fish are to be exported, or remain long unused, they are re-packed on shore, laid in fresh salt, and pressed down hard; without which precaution herrings exported by sea would spoil. The same practice is pursued in France, Hamburgh, and doubtless elsewhere. At Hamburgh, as in Holland, they are packed in the open air, ten packers and three overseers being appointed for the purpose, and all sworn. Besides re-packing, the packers in the maritime towns have also to pick and assort the fish, according to their goodness, salt them anew, and put them in fresh pickle. It is also generally a part of the magistrates and trading companies oaths, not to suffer any bad fish to be delivered from the quay or custom-house.

After

After packing, whether the goods are intended for exportation or home-consumption, whether pressed down hard or not, they are regularly coopered, that the pickle may not leak out, and the fish turn yellow or spoil, which takes place the moment they are deficient in pickle. Properly, the herrings should, on the very day on which they are taken, not only be gutted, but salted and put in casks, or at least should not lie more than one night in the first pickle. And accordingly such herrings are distinguished in France by the name of *Harengs d'une nuit*. But, when the fishery is abundant, this is not always possible; so that only a part can be properly attended to; and the rest, after being gutted, must unavoidably remain, at least the whole following day, if not longer, in the first pickle, the regular packing of them being postponed till the third day. These fish, having stood two nights on deck in the open air, are called *Harengs de deux nuits*. But such goods not only are inferior, but do not keep so well as the former.

If the fishermen mean again to cast their nets on the following night, or if, on account of the great abundance of the shoals, they do not expect to complete the salting and regular packing in two days, the fish, which they cannot so complete, are salted in large heaps, and are then called *Slabbers*, or *Slabbegut*, coarse goods. These are frequently too salt, because want of time prevents their being properly managed. They are put into the schuyts, which always accompany the herring-busses, and washed; after which they are smoked; though not so much as the *Bicklungs* (*Bücklinge*) or red-herrings. The salted herrings, hitherto spoken of, are called *Böckel herringe*, or pickled herrings, or, in general, plainly *Herrings*; those properly salted and packed in layers, *Packed*, or *Barrel herrings*; and those half salted, and promiscuously packed in barrels, *Wreck herrings*.

The other mode of curing, called Red salting, is thus performed. When the fish are taken out of the above-described pickle, in which, however, they must remain longer than those intended for the common, or white-salting, and at least four-and-twenty hours; they are hung by the head in rows on wooden poles, in stoves constructed for the purpose, each of which generally contains 12 000 herrings. Being thus placed, a fire is made under them with vine-stalks, or any green

Dutch method
of catching and
curing herrings.

Dutch method
of catching and
curing herrings.

green faggot-wood, that affords much smoke and little flame.* Here they remain till dried, and properly smoked, which generally requires twenty-four hours. Thus they become Bicklings, or Red-herrings; when these are packed in barrels, they are called Barrelled Bicklings, and are much salted; but, if laid in straw, they are called Straw Bicklings, and are somewhat less salted. The excellence of the Bicklings principally consists in their being large, fat, tender, fresh, properly salted, pliable, soft, of the colour of gold, and not torn or mangled. In Holland, the best fish are chosen for this purpose; but in other places, the above-described Slabbers only are used, or other inferior herrings, deemed unfit for the usual mode of salting. The best and saltiest smoked Dutch herrings are called, in German, *Speckbucklinge*, or fat red-herrings; in low Saxon, *Flickhüringe*; and in Hamburgh, *Flückheeringe*. They are cut open along the back.†

* At Bremen, the place most celebrated for smoking fish, and where no secret is made, as in Holland, of the process, they are hung in ovens of the size of a small parlour, and strict attention is paid not to use fir, or any wood, in which is any the least turpentine or resinous matter, which invariably gives a bad taste to the fish.

† The act of parliament regulations, and many useful observations, may be seen in two small tracts, 8vo, in possession of the Society for the Encouragement of Arts, Manufactures, and Commerce, the one entitled, "Observations on the Herring-Fishery upon the North and East Coasts of Scotland, &c.; with plain Rules, proposed for curing, and for supplying the London Market with White Herrings: By Lewis M'Culloch, many Years employed in furnishing the Merchants of London with Herrings for Exportation. London. Richardson, 1788." The other entitled, "The best and most approved Method of curing White Herrings, and all Kinds of White Fish; containing particular Directions how to slit, gut, salt, dry, and barrel them, fit for Sale at home or foreign Markets; with Directions for boiling of Oil: By a Trader in Fish, London. J. Davidson, 1750."

VII.

The Density of Mercury in its solid State ascertained. By Mr.
JOHN BIDDLE.

To Mr. NICHOLSON.

SIR,

Birmingham, March 4, 1805.

AT the Philosophical Society in Birmingham, of which I am a member, I read a few weeks ago a lecture on mercury. In the course of my experiments on this subject, conducted with a view of exhibiting this metal in a solid state, by exposure to the frigorific mixture of muriate of lime and snow, it appeared to me the specific gravity of this metal would be most accurately obtained in comparison with other metals under similar circumstances of solidity and temperature. I therefore undertook, amongst a variety of others, the following experiments, to ascertain its specific gravity in a solid state, which I believe had never before been accomplished: They succeeded to my satisfaction, and to the gratification of some who were present: I am, therefore, inclined to believe, you will think them worth inserting in your Philosophical Journal.

I am very respectfully yours,

JOHN BIDDLE.

HAVING purified mercury, by distilling from an earthen retort into a glass receiver 50 *per cent.* of the quantity put in, to avoid the alloy of any other metal, and afterwards having heated it to 300° of Fahrenheit, to drive off any water that might adhere, such as was used in other experiments, and exposed to the action of muriate of lime and snow,—I proceeded as follows:

One thousand grains of this mercury were introduced to the freezing-mixture, together with three ounces of alcohol, in a round-bottomed glass; and having placed in the mercury a fine bent wire, the weight of which was previously known when immersed in the same alcohol, to a certain point, in the temperature of 47 degrees above zero of Fahrenheit: this wire, during the congelation of the mercury, was fixed in it, so that

Experiments before the Philosophical Society in Birmingham.

Mercury was purified by distillation;

and exposed to congeal under alcohol.

that the whole became attached to the inner surface of the glass, and could not be separated until the glass with its contents was taken out of the freezing-mixture, and warmed, by dipping it in water, so as to soften the mercury at the surface attached to the glass; one hand being applied to the wire at the same time, to draw the mercury from the glass at the moment of its being loosened: it was then replaced, as before, in the freezing-mixture.

Visible signs of great contraction of the mercury by freezing.

During the congelation of the mercury it was observed, the surface of the metal towards the centre was very considerably depressed by the contraction of its particles, and the vessel having been moved during its congelation, a small hole nearly reaching the bottom of the glass, was observed gradually lessening in its dimensions, presenting a conical cavity with the apex downwards. The wire which held the mercury attached to it was now fixed by the other bent end to the hydrostatic balance, which held in the opposite scale the weights by which the mercury had been weighed in the air, together with the balance of the wire immersed in the same alcohol to a point marked on the wire.

Loss of weight by the immersion.

It appeared by weighing the 1000 grains of mercury thus immersed in alcohol, the loss of weight was 59,8 grains: this was weighed five or six times, with the glass still in the freezing mixture, with the same result; but when it was withdrawn only a little way from the muriate of lime and snow, the difference of weight was perceptible, in consequence of the temperature of the mercury and alcohol increasing.

Comparative experiment with an equal weight of silver.

One thousand grains of pure silver, weighed by the same scale immersed in the same alcohol at the same temperature, lost in weight 88,105 grains: therefore, as the loss of the weight of the mercury is to the loss of weight in the silver, so is the specific gravity of the silver to the specific gravity of the mercury.

Whence the specific gravity of solid mercury is deduced
= 15.612.

The specific gravity of the silver in the same balance having been ascertained, by distilled water, to be 10,436, it follows, if this sum be multiplied into the sum of its loss suspended and weighed in alcohol, and divided by the sum of loss of mercury weighed in the same way, that the specific gravity of the mercury in the solid state, at about 40 degrees below zero on Fahrenheit's scale, equals 15,612.

By

By the same hydrostatic balance it appears, the same mercury in a fluid state, the thermometer standing at 47 degrees above zero, is 13,545. Sp. Gr. when fluid, was 13,545.

By these experiments it appears, the difference of density between its fluid state, at the temperature of 47° above zero, and its solid state, at 40° below zero, is 2,0673 in 13545, or 1,5265 in 10, which is 15,265 per cent. that is, nearly $\frac{1}{4}$ th of its greatest volume, or nearly $\frac{1}{5}$ th of its least volume.

VIII.

Observations on Mr. BOSWELL's Geometrical Propositions. By
AN OLD CORRESPONDENT.

To Mr. NICHOLSON.

SIR,

March 6, 1805.

WHEN your ingenious correspondent Mr. Boswell sent to you his discovery of a ready method of determining a chord-line equal to the side of a square of the same area as the circle in which it is drawn, it should seem that he was not aware that the quantum of error might be ascertained by calculation, as you have done in the subjoined note, otherwise he would not have satisfied himself with the mechanical proof which he has adduced as a test of its accuracy. Had it occurred to you, I am persuaded you would with equal facility have shewn, that this gentleman's other "fact in geometry" is still more erroneous, though announced with a degree of confidence which appears to have arisen from a mistaken conviction of such obvious accuracy as stood not in need of proof.

"A right line (B E), says he, drawn from the extremity B of the line I B, at right angles through the opposite diameter (I F) to the circumference, will be equal to a fourth of the circumference."

Let us try how far this assertion is founded in truth. But before we enter upon the examination, I will beg leave to point out a circumstance in your note, page 152 in your last number, which, having arisen out of inadvertence in your transcribing On Mr. Boswell's lines drawn in a circle.
Overfight by inversion of terms in W. N.'s note.

transcribing it most probably, may tend to puzzle your readers, and among the rest perhaps Mr. Boswell himself, who must be the most interested: You have said, "as the radius is to IW , so is the diameter to IB ," (Plate VIII. Fig. 10); but I take it for granted you meant, "*as IW is to radius, so is the diameter to IB ,*" because this analogy is agreeable to the result of your calculation. And as you have not mentioned how the doctrine of similar triangles applies in the construction of the figure, I hope you and your numerous readers will not deem me presumptuous, if I introduce the diagram again with some additions, particularly as it will give me an opportunity of proving more intelligibly the error of the second proposition already quoted. Any triangle formed in a semicircle, of which the diameter is one side, with its opposite angle at the circumference, is a right angled triangle (Eucl. III. Prop. xxxi.): This will be the case in Mr. Boswell's figure if a right line be drawn from B to F , the right angle being at B ; and as the angle at I is common to the small right angled triangle IOW , and to the large one IBF , the two triangles are similar, and therefore the hypotenuse IW of the small one is to its base IO , as the hypotenuse IF (or diameter) is to the base IB of the large one. Q. E. D.

How far Mr. B.'s second proposition is accurate.

With respect to the second proposition, it is very well known (Eucl. III. Prop. xx.) that the angle formed at the centre of a circle is double of the angle at the circumference, if they have the same common chord as a base; now in the triangle IOW (Plate XI. Fig. 4), the sides IO being given $= .5$, $OW = .25$, and therefore $IW = .55901$, we have, by a simple case in plane trigonometry, the angle at $I = 26^{\circ} 33' 5'' .4$, the double of which is $53^{\circ} 6' 10'' .86$ for the angle BOF at the centre; but BG , one half of the line BE in question, is the sine of this angle, and in natural numbers is, by the tables, equal to .7997163, when unity is radius; but, in our figure, the diameter represents unity, therefore the double sine, or chord line, BE is $= .7997163$, which, according to Mr. Boswell's assertion, ought to be $= .7854$, or one-fourth of the circumference 3.1416 ; hence the error is .0143163, or very nearly $\frac{1}{70}$ th of the whole.

Apology to these remarks.

As I have not the pleasure of knowing Mr. Boswell, notwithstanding I have admired several specimens of his ingenuity, and as I profess to be a promoter of all arithmetical approximations

proximations and scientific projects, that have for their means facility of application, and utility for their object, I mean so far as they have accuracy to recommend them, I trust he will not impute any motive of a personal nature to me, but give me credit for the declaration, when I assert, that the implied invitation collected from his memoir, to ascertain the accuracy of any of his projects, has been my only inducement for laying before the public the present paper.

I am, Sir,

With due respect,

AN OLD CORRESPONDENT.

IX.

A Memoir on Milk and the Lactic Acid. By Cit. BOUILLON
LAGRANGE.

(Concluded from page 144.)

THE muriate of soda, and more especially the muriate of lime, affords a proof of this. These salts effect a separation of the curd only in part; and, nevertheless, the muriate of lime has a very great affinity for water: the decomposition ought therefore, in this case, to be made in a more striking manner than with the acids.

Whey is not therefore the product of fermentation. The alcohol obtained from milk does not prove that this formation is necessary in order to obtain the lactic acid; for this last may be obtained from a serum recently prepared.

With regard to the formation of the carbonic acid indicated in the sixth experiment, it is owing to the decomposition of a small portion of animal matter, and part of the sugar of milk dissolved in the serum. The more of these substances is decomposed the greater will be the quantity of carbonic acid formed, and accordingly the degree of acidity is more marked on account of the presence of the carbonic acid; but if the acid be expelled, the serum is milder, and not so sour as that which has been for some time exposed to the air.

If the serum be exposed to the air for ten or twelve days, in proportion as the saccharine matter is decomposed the whey

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becomes

becomes more acid. These degrees of acidity are owing to a small quantity of acetic acid, formed by the aid of carbonic acid and alcohol remaining in the liquor, which must naturally augment the acidity of the acid already formed in the milk.

Whether the salts in whey render it sour.

With regard to the second question, Whether whey exhibits acid properties only on account of the salts it holds in solution?

I have proved that the acid may be separated from the saline matters without exposing the milk to the air for a certain time, as Scheele has directed; and the experiments I shall relate in this memoir will afford additional proofs. So that its acid properties do not depend merely upon the salts it holds in solution, and the lactic acid is not produced by a fermentation similar to the acetous.

Zoonic acid by distilling milk;

Though the action of caloric upon milk is generally known, I shall nevertheless make an observation upon the acid obtained by distillation with a naked fire, which has for several years been known by the name of the Zoonic Acid.

is acetous acid.

If this acid be purified and combined with potash, a salt is obtained, which presents all the characters of the acetate of potash.

Curd contains phosphate of lime.

I must also observe the cheesy matter in which some chemists have been unable to find phosphate of lime, as announced by Scheele. To obtain this, nothing more is necessary than strongly to heat this substance in a crucible after having well washed it, and to calcine it to whiteness.

The residue is a hard white matter, of which some small pieces, veined with blue, may be compared to the turquois.

The nitric acid dissolves it without effervescence; lime-water and ammonia occasion a flocculent precipitate of phosphate of lime.

The oxalate of ammonia also demonstrates the presence of lime.

SECTION III.

Concerning the Serum of Milk.

Examination of whey separated by different agents.

Being desirous of knowing whether there is any difference between the wheys obtained from milk by different substances, I made use of the mineral acids, some vegetable acids, salts, such as alum, the acidulous tartrate of potash, &c. The coagulating substance was employed in due quantity; as I had remarked,

remarked, that when an excess of acid is used, it is found to exist in the serum; whereas, when the necessary quantity is used, it is found in the cheese only.

After having separated the cheesy matter the serum was clarified; the wheys were found to have all the same taste and colour, and possessed the same degree of acidity. It is the same fluid in the different cases.

This fresh whey turns syrup of violets green: This property must be attributed only to the union of the yellow and blue, and not to saline matters, as chemists have asserted. Whey, which has lost its colour by exposure to the air, reddens the syrup of violets. It affects syrup of violets.

The means employed to ascertain the acids which had been used to coagulate the milk, did not exhibit even the smallest quantity when the operation was performed with exactness.

I made a comparative examination of the serum which had become sour in the open air; and I obtained, by exposing the milk, the same phenomena as were pointed out in the second section and third experiment. Sour serum by exposure of milk

The spontaneous serum differed, 1. In its taste, which was more acid; and, 2. Its colour, which is dull and whitish, owing to white filaments which are separated. It may be had clear by filtration, without acquiring the colour of fresh whey. It differed from other serum.

The cheesy matter resulting from the several experiments before related, gave the same red with tincture of turnsole. If it be calcined and treated with the known re-agents, the presence of sulphuric acid will be indicated if this acid has been employed in the coagulation, and alumine when alum has been made use of. Cheesy matter

I do not therefore see any inconvenience, in preparing whey for the purposes of pharmacy, to coagulate the milk with one or the other of these substances, more especially if the precautions I have mentioned be attended to. Those who have thought it unwholesome to coagulate milk with alum, have doubtless made no experiment on the subject. Alum or sulphuric acid may be employed to coagulate milk.

Oxygen gas is not absorbed by the serum; even agitation does not facilitate its union. Oxygen and serum.

The action of lime-water, the solution of barytes, strontian, and some metallic salts, though already known by chemists, require to be again examined, and will enable me to explain the processes for obtaining lactic acid. Other agents.

SECTION IV.

On the Sugar of Milk.

Whether acetic acid in milk arise from the decomposition of its sugar.

The formation of acetic acid does not arise merely from the alteration of the sugar of milk; and though the experiments already described admit of certain conjectures, I have ascertained whether a like quantity of sugar of milk could be obtained from fresh serum and a serum exposed for twelve days at a temperature from 14 to 20 degrees.

A little sugar is decomposed by standing.

I have found that four milk affords a somewhat less quantity of sugar of milk, which proves, that a portion of the saccharine matter was employed in forming a quantity of acetic acid.

We may therefore obtain carbonic acid, alcohol, and acetic acid, either by confining whey in close vessels, or by leaving it exposed to the air.

Other experiments made on pure sugar of milk indicated nothing remarkable.

Habitudes of sugar of milk.

It does not redden tincture of turnsole. When dissolved in water and exposed for a long time to the air, it does not become acid. By distillation it affords acetic acid along with the known products.

It is soluble in weak acetic acid. If a small quantity of fresh gaseous matter be added, it is soon found that the liquor holds it in solution. By pouring in a few drops of alkali to saturate the excess of acid, it loses its transparency, becomes turbid and milky, with a mild taste resembling that of milk, and preserves its opacity for a considerable length of time.

General results respecting milk.

The following results are deducible from the preceding facts:—

1. New milk reddens the tincture of turnsole.
2. The cheesy matter may be separated without contact of the air.
3. The serum does not retain the acids made use of to coagulate milk.
4. Distillation either of milk or of sugar of milk to dryness, affords the acetic acid.
5. The formation of carbonic acid and alcohol is owing to muco-saccharine matter.
6. A quantity of acetic acid is formed by the fermentation of these substances.

7. The

7. The cheesy matter, when separated, always manifests the presence of an acid, and differs in taste and consistence according to the matters employed in its separation.

8. When washed and affording no sign of acidity, if it be afterwards diffused in distilled water for several days, at a temperature between 15 and 22 degrees (about 65 and 80 Fahrenheit); it acquires a strong disagreeable odour; the fluid slightly reddens turnsole, and lime disengages ammonia.

9. The serum and the cheesy matter contain, besides the known substances, phosphate of lime, as Scheele had announced.

10. The difference which exists between fresh serum and that which has been exposed to the air, consists in the acetic acid, which is present along with the free acid in the milk.

11. Lastly, there exists in milk and the serum a free or uncombined acid, which seemed to me to be the acetic acid.

SECTION V.

Concerning the Lactic Acid.

Scheele having ascertained that it is not possible to obtain the acid by simple distillation, had recourse to peculiar methods to effect this separation.

This chemist first reduced the whey to one-eighth by evaporation; he then filtered it, and there remained, according to him, no more cheesy matter.

He saturated the fluid with lime-water, and phosphate of lime was precipitated: This liquor having been filtrated and diluted with three times its quantity of water, he poured in oxalic acid drop by drop, to seize and precipitate all the lime; and he ascertained, by the addition of a little lime-water, that no more oxalic acid remained. He evaporated the liquor to the consistence of honey; the thickened acid was redissolved in rectified alcohol; the sugar of milk and all the foreign substances remained on the filtre, not having been dissolved by the alcohol. Lastly, after having again added a little water to the acid held in solution by the alcohol, he drew off this last fluid by distillation, and found the lactic acid in the retort.

This long and expensive process does not afford a pure acid. It was therefore necessary to adopt a simpler process before its nature was examined.

Experiment

With fresh whey little acid is obtained.

Experiment 1. If instead of exposing whey to the air as Scheele directs, the process be immediately performed on the fresh whey, a very small quantity only of acid is obtained, which is less coloured and has a more animal smell.

This acid appeared to me to be uncombined in the milk, as its quantity was so small.

If barytes or strontian and sulphuric acid be used instead of the lime-water and oxalic acid of Scheele the result is equally perfect. Process with acetate of lead unsuccessful.

Experiment 2. Instead of using lime-water and the oxalic acid, I used barytes or strontian and the sulphuric acid. By this means I obtained an acid no less pure than that of the Swedish chemist.

Experiment 3. I had likewise attempted to decompose a lactate of potash with acetate of lead, expecting to form a lactate of lead decomposable by the sulphuric acid; but the precipitate thus formed is not a lactate of lead, for when a sufficient quantity of sulphuric acid is added for the purpose of decomposing it, no uncombined acid is set at liberty. It appears that part of the oxide falls down with the animal matter; and this compound is even to a certain degree soluble. It is also found that the fluid always retains much acetate of lead in solution.

We therefore see that though this process has been pointed out as superior to that of Scheele, it is certain that it cannot be employed to obtain the lactic acid. The following experiments were made on the lactic acid of Scheele.

Lactic acid of Scheele examined.

1. The intensity of its colour may be diminished by grinding a piece of red hot charcoal in a mortar, and pouring the lactic acid upon it. A quantity of small flakes were separated which rose to the surface. The fluid was slightly boiled and filtered, and the acid was then less coloured, and had less odour.

2. When this acid is distilled in a retort acetic acid passes over and a thick yellow very acid matter remains.

3. It may perhaps be thought that part of this acid becomes changed into acetic acid by the assistance of caloric, as Berthollet observes in his Chemical Statics.

“The ternary acids, as this chemist observes, may be changed into acetic acid, when by the action of heat they abandon part of their carbon, and their elements, which resist this action less, are separated by volatilization.”

I have ascertained that great part of this acid previously existed in the fluid.

3. The same phenomena take place, but more strikingly, if whey be distilled which has been exposed to the air at the temperature of 15 or 20 degrees (60 and 68 Fahrenheit) for about a fortnight. The first product is a clear transparent fluid of an acid taste, and odour of alcohol, which when combined with potash, forms an acetate.

If alcohol be added to what remained in the retort, it becomes coloured. After decantation and distillation, an acid is found in the retort of a peculiar odour, animalized, and resembling the preceding; but the acid contains a very small quantity of phosphate of lime.

These experiments confirm the observations I have made: It is a mixed acid.

1. That a mixed acid is obtained by the process of Scheele, and 2. That an acid exists in milk and in whey recently prepared. It remains to shew the nature of that acid. For that purpose I prepared the lactic acid according to the process of Scheele with every necessary precaution. I even ascertained the purity of my alcohol, which having been distilled from muriate of lime, marked 40 degrees of the aerometer of Reaumur.

The lactic acid when dissolved in alcohol was submitted to distillation. There passed into the receiver an alcohol which reddened the tincture of turnsole.

When dissolved in alcohol some acid comes over with the spirit.

When the acid was totally deprived of alcohol the distillation was stopped. The lactic acid of Scheele remained of a yellow colour and very sour taste.

Lactic acid remained.

Experiment 1. I mixed quick lime with this concentrated acid, and a disengagement of ammonia took place.*

It gave out ammonia by lime.

Experiment 2. If a lactate of potash be formed and evaporated by a gentle heat, a brown matter is separated, which is insoluble in water. This being afterwards heated in a crucible, the salt swells up and emits an animal odour. The presence of prussic acid was proved by dissolving the remaining matter in water and adding a small portion of the sulphate of iron in solution.

Lactate of potash leaves prussic acid by heat.

* I had before ascertained the presence of ammonia in this acid. I announced it on the 15th Nivose to the Society of Pharmacy. Mr. Vauquelin, who was not acquainted with this fact, informed me at the reading of the first part of this memoir that he had also ascertained the presence of an animal matter in the lactic acid of Scheele.

Experiment

Sulph. acid
added to lactic
acid disengages
acetic acid.

Experiment 3. If sulphuric acid be added to lactic acid, or to a lactate of potash, a disengagement takes place not only of acetic acid, but also of an elastic fluid, which forms a thick cloud when in contact with ammonia.

Distillation.

Experiment 4. When the preceding mixture is distilled in an apparatus proper to receive the gaseous product in water, the distilled water of the bottle preserves its transparency; the smell of acetic acid is very evident; it reddens turnsole and precipitates the muriate of silver.

When a small quantity of ammonia was brought near the tube out of which the gas issued, a very dense cloud was formed.

Fire decomposed
the acid of the
lactate of potash.

Experiment 5 What was left in the retort was evaporated to dryness. This residue was brown, very acid, and when afterwards heated in a crucible of platina, swelled up and left a coal.

During this operation a small quantity of sulphuric acid was decomposed.

The matter that remained in the retort was dissolved in distilled water. The solution did not redden turnsole, but it gave a brown colour to paper tinged with curcuma. A little sulphuric acid was added to saturation, and after evaporation crystals were obtained by cooling, which exhibited the characters of sulphate of potash,

As the supernatant liquor had a metallic taste, prussiate of lime was poured on it, which produced a bluish tinge.

Lactic acid de-
composed by fire
leaves potash.

Experiment 6. The lactic acid may also be concentrated to dryness; heated to incineration in a crucible; afterwards dissolved in a distilled water and precipitated by nitrate of silver. A muriate of silver is formed which may be separated by the filter; and the fluid which contains the nitrate is then evaporated and decomposed by heat. A white matter remains, which when dissolved in water, affords with the tartareous acid an acidulous tartrate of potash.

Component parts
of Scheele's
lactic acid.

Hence I conclude that the lactic acid of Scheele is composed of acetic acid; muriate of potash; a small portion of iron probably dissolved in the acetic acid; and an animal matter.

X.

An Analysis of the Magnetical Pyrites; with Remarks on some of the other Sulphurets of Iron. By CHARLES HATCHETT, Esq. F. R. S. From the *Philosophical Transactions* for 1801.

§ I.

OF the various metallic sulphurets which constitute one of the grand divisions of ores, none appear to be so universally diffused throughout the globe, as the sulphuret of iron, commonly called Martial Pyrites; for the species and varieties of this are found at all depths, and in all climates and soils, whether ancient, or of alluvial and recent formation. It is remarkable also, that, under certain circumstances, this sulphuret is daily produced in the humid way; an instance of which, a few years back, I had the honour, in conjunction with Mr. Wiseman, to lay before this Society; * and although, in regard to pecuniary value, the pyrites of iron may be considered as comparatively insignificant, yet there is every reason to believe, that in the operations of nature, it is a substance of very considerable importance.

§ II.

The species and varieties of martial pyrites, are in general so well known, and have been so frequently and accurately described, as to figure, lustre, colour, and other external characters, that it would be totally superfluous here to give any detailed account of them. One of the species, however, merits peculiar notice, as possessing the remarkable property of strong magnetic polarity; and, although it has been described by modern mineralogists,† it does not appear to have been as yet subjected to any regular chemical examination; so that, whether it be a sulphuret of iron inherently endowed with the magnetical property, or a sulphuret in which particles of the ordinary magnetical iron ore are simply but minutely interspersed, has to this time remained undecided.

* Phil. Transf. for 1798, p. 567.

† Kirwan, Vol. II. p. 79. Widenmann, p. 793. Emmerling, 2d edit. Tom. II. p. 286. Karsten, p. 48. Brochant, Tome II. p. 232.

Its obvious or
external prop-
erties.

This species is known by the name of Magnetical Pyrites, and is called by the Germans *Magnet-Kies*, or *Ferrum mineralisatum magnetico-pyritaceum*.

It is most frequently of the colour of bronze, passing to a pale cupreous-red.

The lustre is metallic,

The fracture is unequal, and commonly coarse-grained, but sometimes imperfectly conchoidal.

The fragments are amorphous.

The trace is yellowish-gray, with some metallic lustre.

It is not very hard; but, when struck with steel, sparks are produced, although with some difficulty.

It is brittle, and is easily broken.

Where found,

This pyrites has been hitherto found only in some parts of Norway, Silesia, Bavaria, and especially at Geier, Mefferdsdorf, and Breitenbrunn in Saxony; but, having received some specimens from the Right Hon. Charles Greville, F. R. S. I was struck with their resemblance to the pyrites of Breitenbrunn, which happened at that time to be in my possession; and, upon trial, I found that they were magnetical, and agreed with the latter in every particular. Their magnetic power was such as strongly to affect a well-poized needle, of about three inches in length; a piece of the pyrites, nearly two inches square, acted upon the needle at the distance of four inches.

Degrees of
magnetism.

The powder (which is blackish-gray, with but little metallic lustre) is immediately taken up by a common magnet; but the pyrites does not act thus on the powder, nor on iron filings, unless it has been placed for some time between magnetical bars; then indeed it acts powerfully, turns the needle completely round, attracts and takes up iron filings, and seems permanently to retain this addition to its original power.

In the specimens which I obtained, the north pole was generally the strongest.

This pyrites was found in Wales, about the year 1798, by the Hon. Robert Greville, F. R. S. who sent the specimens above described to his brother, the Right Hon. C. Greville, with the following account.

Abundantly
found in Caer-
narvonshire.

" It is found in great abundance in Caernarvonshire, near the base of the mountain called Moel Elion, or probably with more accuracy Moel Ælia, and opposite to the mountain
" called

MAGNETICAL PYRITES.

" called Mynydd Mawr. These mountains form the entrance
" into a little close valley, which leads to Cywellin lake, near
" Snowdon, a little beyond the hamlet of Bettws.

" The vein appears to be some yards in depth and breadth,
" and seems to run from north to south, as it is found on
" Mynydd Mawr, which is across the narrow valley, and
" opposite to Moel Ælia."

Mr. R. Greville, in another part of his letter, states that
copper ore has been worked in several of the adjacent places,
and that, many years ago, Capt. Williams, of Glan yr Avon,
employed some miners at the place where this pyrites is found,
but the undertaking proved unproductive. Yellow copper ore
is certainly in the vicinity; for some portions of it were adhering
to the specimens which have been mentioned; and I shall here
observe, that the stone which accompanies the magnetical
pyrites, is a variety of the lapis ollaris or pot-stone, of a pale
grayish-green, containing smooth cubic crystals of common
pyrites.

§ III.

From the appearance of those parts of the magnetical pyrites which have been exposed to the weather, it seems to be liable to oxidizement, but not to vitrification.

Magnetic pyrites is slightly oxidable by the weather.

The specific gravity, at temperature 65° of Fahrenheit, is Sp. gravity.
4518.

When exposed to the blowpipe, it emits a sulphureous odour, and melts into a globule nearly black, which is attracted by the magnet.

Effect of mere heat by blow-pipe:

Five hundred grains, in coarse powder, were exposed, in a small earthen retort, to a red heat, during three hours. By this operation, the weight of the powder was very little diminished; neither was there any appearance of sulphur in the receiver, which however smelt strongly of sulphureous acid.

Five hundred grains of the same were put into a flat porcelain crucible, which was kept in a red heat, under a muffle, during four hours. The powder then appeared of a dark gray, with a tinge of deep red, and weighed 432.50 grains. The loss was therefore 67.50 = 13.50 per cent. but, upon examining the residuum, I found that only part of the sulphur had been thus separated.

The

Partly soluble
in dilute sulphuric
acid.

The magnetical pyrites, when digested in dilute sulphuric acid, is partially dissolved, with little effervescence, although there is a very perceptible odour of sulphuretted hydrogen.

The solution is of a very pale green colour.

Precipitates,

Pure ammonia produced a dark green precipitate, tending to black; and prussiate of potash formed a very pale blue precipitate, or rather a white precipitate mingled with a small portion of blue. The whole of the latter, however, by exposure to the air, gradually assumed the usual intensity of Prussian blue; and the blackish green precipitate, formed by ammonia, became gradually ochraceous. These effects therefore fully prove, that the iron in the solution was, for the greater part, at the minimum of oxidizement, so as to form the green sulphate, and white prussiate, of iron; * and, consequently, that the iron of the magnetical pyrites is either quite, or very nearly, in the state of perfect metal.

Effect of nitric
acid.

This pyrites, when treated with nitric acid, of the specific gravity of 1.38, diluted with an equal quantity of water, is at first but little affected; but, when heat is applied, it is dissolved, with much effervescence, and discharge of nitrous gas; the effervescence, however, is by no means so violent as when the common pyrites are treated in a similar manner. It is also worthy of notice, that if the digestion be not of too long duration, a considerable quantity of sulphur, *in substance*, is separated; whilst, on the contrary, scarcely any can be obtained from the common pyrites, when treated in a similar manner; although I shall soon have occasion to prove, that the real quantity of sulphur is much more considerable in the latter than in the former.

Of muriatic
acid.

As soon as muriatic acid is poured on the powder of the magnetical pyrites, a slight effervescence is produced, which becomes violently increased by the application of heat; a quantity of gas is discharged, which, by its odour, by its inflammability, by the colour of the flame, by the deposition of sulphur when burned, and by other properties, was proved to be sulphuretted hydrogen.

During the digestion, sulphur was deposited, which so enveloped a small part of the pyrites, as to protect it from the farther action of the acid.

* Recherches sur le Bleu de Prusse, par M. Proust. Annales de Chimie, Tome XXIII. p. 85.

The solution was of a pale yellowish-green colour. With **Muriatic solution.** prussiate of potash it afforded a pale blue precipitate, or rather a white precipitate mixed with blue; and with ammonia it formed a dark blackish-green precipitate, which gradually became ochraceous; so that these effects corroborated the conclusions which were founded on the properties of the sulphuric solution, namely, that the iron contained in the pyrites, is almost, if not quite, in the metallic state.

Other experiments were made; but, as they merely confirm the above observations, I shall proceed to give an account of the analysis.

§ IV.

ANALYSIS OF THE MAGNETICAL PYRITES.

A. One hundred grains, reduced to a fine powder, were digested with two ounces of muriatic acid, in a glass matrass placed in a sand bath. The effects already described took place; and a pale yellowish-green solution was formed. The residuum was then again digested with two parts of muriatic acid mixed with one of nitric acid; and a quantity of pure sulphur was obtained, which, being dried, weighed 14 grains.

Analysis.
100 gr. magnetic pyrites were digested in muriatic acid. Partial solution. Residue treated with nitro-muriatic acid. Some sulphur separated.

B. The acid in which the residuum had been digested, was added to the first muriatic solution; some nitric acid was also poured in, to promote the oxidizement of the iron, and thereby to facilitate the precipitation of it by ammonia, which was added after the liquor had been boiled for a considerable time. The precipitate thus obtained was boiled with lixivium of potash; it was thenedulcorated, dried, made red-hot with wax in a covered porcelain crucible, was completely taken up by a magnet, and, being weighed, amounted to 80 grains.

To the acid solution was added some nitric acid and the iron was then precip. by ammonia. The precip. was boiled with potash and then reduced to black oxide.

C. The lixivium of potash was examined by muriate of ammonia, but no alumina was obtained.

The potash lixivium gave no alumina.

D. To the filtrated liquor from which the iron had been precipitated by ammonia, muriate of barytes was added, until it ceased to produce any precipitate; this was then digested with some very dilute muriatic acid, was collected, washed, and, after exposure to a low red heat for a few minutes in a crucible of platina, weighed 155 grains. If therefore the quantity of sulphur, converted into sulphuric acid by the preceding operations, and precipitated by barytes, be calculated according to the accurate experiments of Mr. Chenevix, these 155 grains of sulphate

The first solution liquor was precip. by muriate of barytes. The sulphate of barytes thus obtained shewed the quantity of sulph. acid.

fulphate of barytes will denote, nearly, 22.50 of sulphur ; so that, with the addition of the 14 grains previously obtained in substance, the total quantity will amount to 36.50.

Estimate of the pure iron in the oxide.

E. Moreover, from what has been stated it appears, that the iron which was obtained in the form of black oxide, weighed 80 grains; and, by adding these 80 grains to the 36.50 of sulphur, an increase of weight is found = 16.50. This was evidently owing to the oxidizement of the iron, which, in the magnetical pyrites, exists quite, or very nearly, in the metallic state, but, by the operations of the analysis, had received this addition. The real quantity of iron must, on this account, be estimated at 63.50.

Component parts.
Sulphur 4. and iron 7.

One hundred grains, therefore, of the magnetical pyrites, yielded,

$$\begin{array}{rcl} \text{Sulphur} & \left\{ \begin{array}{l} \text{A. } 14 \\ \text{D. } 22.50 \end{array} \right\} & 36.50 \text{ grains.} \\ \text{Iron} & \text{E.} & = \quad 63.50 \\ & & \hline & & 100. \end{array}$$

Analysis repeated by acidifying all the sulphur by nitric acid. Same result.

This analysis was repeated in a similar manner, excepting that the whole was digested in nitric acid, until the sulphur was intirely converted into sulphuric acid. To the liquor which remained after the separation of the iron by ammonia, muriate of barytes was added, as before, and formed a precipitate which weighed 245 grains. Now, as the sulphuric acid in fulphate of barytes is estimated by Mr. Chenevix at 23.5 *per cent.* and the sulphur which is required to form the sulphuric acid contained in 100 parts of fulphate of barytes, at 14.5 *, it follows, that 245 grains of dry fulphate of barytes, contain sulphuric acid equal, very nearly, to 36 grains of sulphur ; so that the two analyses corroborate each other. The proportion of sulphur in the magnetical pyrites, may therefore be stated at 36.50, or indeed at 37 *per cent.* if some small allowance be made for the occasional presence of earthy particles; a minute portion of quartz having been found, by the last analysis, after the complete acidification of sulphur.

The iron in this and in all pyrites, is very nearly in the metallic state.

The increase produced, by the operations of the analysis, in the weight of the iron, arose, as I have already remarked, from the addition of oxygen ; for the iron, as obtained by the analysis, was in the state of black oxide ; but in this, and

* Transactions of the Royal Irish Academy, Vol. VIII. p. 240.

indeed in all pyrites, it undoubtedly exists very nearly, or quite, in the state of perfect metal. Now the black oxide of iron, called Protoxide by Dr. Thomson *, has been proved, by Lavoisier and Proust, to consist of 100 parts of metallic iron combined with 37 of oxygen, thus forming 137 of black oxide; the exact proportion of oxygen is therefore 27 per cent, and 80 grains of this oxide must contain 21.6 of oxygen. But, in the above analyses of the magnetical pyrites, the increase of weight did not amount to more than 16.5; and we may therefore conclude that, in all probability, a quantity of oxygen = 5.1 was previously combined with some part, or with the general mass, of the iron in the pyrites. A small part of the abovementioned increase of weight, must likewise have arisen from another cause; for, although the true proportions of the black oxide of iron are 27 of oxygen and 73 of iron, (so that 100 parts of the latter absorb 37 of the former,) yet, in actual practice, it is difficult to obtain it exactly in this state, and there is commonly a small excess of weight: this I have repeatedly observed, in many experiments, some of which were purposely made. When, for instance, 100 parts of fine iron wire were dissolved in muriatic acid, and afterwards precipitated by ammonia, edulcorated, dried, and made red-hot with a small quantity of wax in a covered porcelain crucible, the weight, instead of 137, usually amounted to 139 or 140. The quantity of wax employed, certainly did not afford a ponderable quantity of coal, or other residuum; but the real cause of the increase of weight, appears to be the air, which can scarcely be completely excluded, and which, after the wax is burned, combines with the superficial part of the oxide, and converts a portion of it into the red or peroxide; so that the surface in the crucible appears brown, when compared with the interior.

To this cause, therefore, I am inclined also to attribute a small part of the increase observed in the weight of the iron obtained by the preceding analyses.

§ V.

Before I make any observations on the nature of the sulphuret which has been proved to constitute the magnetical

* System of Chemistry, 2d edition, Vol. I. p. 147.

Comparative analysis of other pyrites, by adding the whole of the sulphur.

pyrites, it may be proper to state some comparative analyses which I have made of several of the common pyrites; and, as the method employed was precisely the same as that which has been described, all that seems to be requisite, is to give an account of the results.

In each analysis, the whole of the sulphur was converted into sulphuric acid, which was precipitated by barytes; and, in the selection of the specimens, great attention was paid to take the internal parts of the fragments, and not to make use of any which exhibited an appearance of decomposition, or of extraneous substances.

The iron was, as before, reduced to the state of black oxide; and the addition of weight in each separate analysis, corresponded, within a few fractional parts, with the proportion of oxygen requisite to form into black oxide a given quantity of metallic iron, equal to that which in each pyrites was ascertained to be the real proportion, by deducting the quantity of sulphur from the total quantity of each pyrites.

The iron, therefore, in these is completely metallic, and as such is stated in the following results:

The component parts of the common pyrites were, at a medium, 9 sulphur and 8 iron, with no great variation.	No. 1. Pyrites in the form of dodecaedrons with pentagonal faces. - -	} Sulphur 52.15 Iron 47.85
	Specific gravity 4830.	
		100.
	No. 2. Pyrites in the form of striated cubes.	} Sulphur 52.50 Iron 47.50
		100.
	No. 3. Pyrites in the form of smooth polished cubes, found in the lapis ollaris which accompanies the magnetical pyrites. -	} Sulphur 52.70 Iron 47.30
	Specific gravity 4831.	
		100.
	No. 4. Radiated pyrites. - -	} Sulphur 53.60 Iron 46.40
	Specific gravity 4698.	
		100.
	No. 5. A smaller variety of radiated pyrites.	} Sulphur 54.34 Iron 45.66
	Specific gravity 4775.	
		100.

Considering the difference in the figure, lustre, and colour of these pyrites, I expected to have found a much greater difference

difference in the proportions of their component ingredients; but, as the results are the average of several experiments, I have not any reason to doubt their accuracy.

The pyrites crystallized in regular figures, such as cubes and dodecahedrons, according to the above analyses, contain less sulphur, and more iron, than the radiated pyrites, perhaps than others which are not regularly crystallized. This difference, however, is not considerable; for the dodecaedral pyrites, which afforded the smallest quantity of sulphur of any of the regularly crystallized pyrites, yielded 52.15; and the radiated pyrites, No. 5, gave 54.34; the difference, therefore, is only 2.19. So that the mean proportion of sulphur, in all the pyrites which were examined, is 53.24 per cent. and, taking the proportion of sulphur in the magnetical pyrites at 56.50 or 57, the difference between this and the mean of the common pyrites will be 16.74 or 16.24. The magnetical pyrites, therefore, is quite distinct, as a sulphuret of iron, from the common martial pyrites; and, in the following observations I shall prove, that a sulphuret consisting of the proportions last mentioned, has till now been unknown as a product of nature.

§ VI.

Although pyrites is one of the most common of mineral substances, yet the discovery of its real nature is comparatively of a late date; for it appears that even Agricola (whose knowledge of mineral bodies was certainly great, considering the state of science in his time) was not acquainted with its characteristic ingredient, namely, iron. According to Henckel, this was first noticed by our countryman Martin Lister, a member of this learned Society, who says, "*Pyrites purus putus ferri metallum est.*"

From the time of Henckel, pyrites seems little to have attracted the notice of chemists, until Mr. Proust, the learned professor of chemistry at Madrid, published two memoirs, in which he states, that there are two sulphurets of iron, the one being artificial, and the other natural. The first is the sulphuret which is formed in laboratories, by adding sulphur to red-hot iron, or by exposing both of them to heat in a retort. This is distinguished from the second sulphuret, (which is the common martial pyrites,) by its easy solubility in acids, especially

in muriatic acid, by the formation of sulphuretted hydrogen gas during the solution of the sulphuret in the last named acid, by its colour, and by its inferior density.

Artificial sulphuret, 6 sulphur and 10 iron; natural, 9 sulphur and 10 iron.

The excess of sulphur is easily separated from pyrites; but not the other portion.

According to Mr. Proust, the first or artificial sulphuret is composed of 60 parts of sulphur, combined with 100 parts of iron; whilst the second sulphuret, or common pyrites, consists of 90 parts of sulphur and 100 of iron.

He moreover observes, that the sulphur of the first sulphuret is difficultly separated; but that the excess which is in the second sulphuret, or common pyrites, is easily expelled, and is that portion which is obtained by distillation, the residuum being then reduced to the state of the first sulphuret *. 100 parts, therefore, of this substance, are composed of 62.50 of iron and 37.50 of sulphur; and 100 parts of common pyrites are, according to this statement, composed of 52.64 of iron and 47.36 of sulphur.

They are the maxima and minima of the sulphurets.

These proportions Mr. Proust considers as the minimum and maximum of the sulphurets of iron. For the latter he allows some variation; but the composition of the former he regards as fixed by the invariable law of proportions †; although he observes, that it has not as yet been discovered in the mineral kingdom ‡.

In support of these assertions, Mr. Proust states,

Experiments of Proust on this subject.

1. That the pyrites found near Soria, when distilled in a retort heated to redness, afforded nearly 20 per cent. of sulphur.

2. That the residuum of the above distillation had lost the external characters and chemical properties of pyrites, and had assumed those of the artificial sulphuret of iron.

* *Journal de Physique*, Tome LIII. p. 89, and Tome LIV. p. 89. From pp. 91 and 92 of Tome LIV. it is evident, that the author does not mean to assert, that the first sulphuret contains 60 per cent. of sulphur; but that 100 parts of iron are combined with 60 of sulphur, and form 160 of the sulphuret. In like manner, when 90 of sulphur are united with 100 of iron, a substance analogous to common pyrites is formed, which weighs 190 grains or parts.

See also our Journal, I. 109, 269, 253, for translations of these excellent memoirs.

† *Journal de Physique*, Tome LIII. p. 90.

‡ "Le regne minéral, jusqu'ici, ne nous a point encore présenté le fer sulphuré au minimum." *Journal de Physique*, Tome LIV. p. 93.

3. That when to this residuum a quantity of sulphur was added, and the whole was distilled in a degree of heat not too great, the 20 *per cent.* of sulphur, which had been separated by the first distillation, was, by this, again restored; and the mass in the retort thus recovered nearly the original colour, lustre, and chemical properties of the pyrites.

4. That, by adding sulphur to iron filings, or fine iron wire, heated to a low red in a retort, a compound is obtained, in which the proportion of sulphur amounts only to about 20 or 30 parts; but, if this compound is again treated with sulphur in a red heat, a sulphuret is formed, which is readily dissolved in acids, and plentifully affords sulphuretted hydrogen gas.

This is the real minimum of the sulphurets of iron, fixed by the invariable law of proportions, (according to Mr. Proust,) at 59 or 60 of sulphur and 100 of iron, the former being (as I have already observed) in the proportion of 37.50 *per cent.*

5, and lastly. That when this sulphuret is again mixed and distilled with sulphur, (due attention being paid to the degree of heat,) the product is found to have assumed most of the chemical and external properties of the natural common pyrites, density alone being excepted.

The application of the above observations, to the principal subject of the present paper, is sufficiently obvious; for, when it is considered, that the magnetical pyrites is so different from the common pyrites, in colour, hardness, solubility in sulphuric acid, and more especially in muriatic acid, with the copious production of sulphuretted hydrogen gas; when, by analysis, it has been found to consist of 36 or 37 of sulphur, combined with about 63 of metallic iron; and, when the artificial sulphuret of iron which has been lately described, is proved to agree with the magnetical pyrites in the nature and proportions of its component ingredients, and in every one of the above-mentioned properties; it is evident that the magnetical pyrites is identically the same with this sulphuret, which hitherto has remained undiscovered in nature, and has only been known as a product of our laboratories. In order however more fully to satisfy myself, I made experiments on the artificial sulphuret, which I formed with sulphur and fine iron wire.

The magnetical pyrites is that which Proust considered as having been only produced by art.

The artificial
pyrites agreed
with the natural
magnetic pyrites.

This substance agreed, in all the properties which have been noticed, with the magnetical pyrites; and the precipitates obtained by adding prussiate of potash, and ammonia, to the muriatic and sulphuric solutions, were precisely similar. The specific gravity was 4390, whilst (as I have already remarked) that of the magnetical pyrites is 4518.

(To be concluded.)

XI.

Answer to a Letter of C. L. with other Remarks, on the Images formed by convex Lenses. In a Letter from Mr. EZEKIEL WALKER.

TO Mr. NICHOLSON.

DEAR SIR,

Remarks on the
Letters of C. L.

C. L. in his letter in your last Number, represents my first paper as deficient in "philosophical correctness," and wanting "the calm spirit of philosophy." But in attempting to ~~move~~ ^{prove} my errors, he has proved nothing but his own want of knowledge: and to those who wish to see an example of C. L.'s "*calm spirit of philosophy*," I beg leave to recommend the perusal of his letter in Vol. IX. p. 235, of this Journal.—These strange assertions are followed by another still more strange.

Quotation, in
which C. L. de-
nies Mr. W.'s
facts.

He says, "*I will beg leave to wave any attention to the history of Mr. Walker's experiments upon which I have animadverted, and to deny his facts.*"

Thinking that I did not understand his meaning correctly, I examined the side-note, which runs thus: "C. L. declines any further discussion respecting Mr. Walker's experiments with lenses, *but denies the facts.*"

Mr. W.'s reply.

As there is no reasoning with a man who will deny facts, I shall decline examining the remainder of C. L.'s letter.

Your other anonymous correspondent, who has denied the truth of my experiments, will be more easily answered.

Horizontal
moon.

The opinions which have been advanced respecting the horizontal luminaries, since the days of Ptolemy, who was born about the year 70 of the Christian æra, are so numerous, that

that it is scarcely possible to form a new one. This correspondent has picked up the theory of Des Cartes, which was confuted more than 100 years ago by Mr. Molyneux. See *Philos. Trans. abr.* Vol. I. p. 221; or Dr. Hutton's *Mathematical Dictionary*, Vol. II. p. 74. I shall pass over this writer's optical errors by just observing, that he has been doing nothing less than denying an established property in optics.

But before I proceed, it may be convenient to those who are not in possession of your two Numbers which contain my former papers on this subject, to know what that property in optics is, which has met with so much opposition. Farther remarks on the image turned by a lens,

I have proved experimentally, that the image of a candle in the focus of a double convex lens, decreases in magnitude as the aperture of the lens is contracted*: And I have also advanced, that the image of the sun or moon, in the focus of the object-glass of a telescope, decreases with its aperture †. asserted to vary with the aperture.

Now it is well known to those who understand optics, that this property, the truth of which has been denied so positively, was discovered by Sir Isaac Newton: and a demonstration of it may be seen in the *Optics* of Newton, Emerson, Smith, and Martin, and also in the *Encyclopædia Britannica*, &c. This position ascribed to Newton,

"The breadth of the least circular space into which object-glasses of telescopes can collect all sorts of parallel rays, is about the $27\frac{1}{2}$ part of half the aperture of the glass, or 55th part of the whole aperture." (Newton's *Optics*, 2d Edit. p. 73). Hence it is evident, that if you contract the breadth of the object-glass from 10 to 1, the breadth of the circle of light, in its focus, will be contracted in the same proportion. because he assumes the space of diffusion in pencils of rays. Whence it is deduced, that the image also will vary.

In my next paper, this interesting property of light will be further explained.

I am, Dear Sir,

Your humble servant,

EZ. WALKER.

Lynn, March 16, 1805.

* *Philosophical Journal*, Vol. IX. p. 164.

† *Philosophical Journal*, Vol. X. p. 110.

XII.

*Account of an Aerostatic Voyage, made by M. GAY-LUSSAC on the 29th Fructidor, in the Year 12. Read to the National Institute, on the 9th Vendemiaire, in the Year 13.**

Motives of the ascent.

IN the account of the first aerostatic voyage which M. Blot and I had the honour to submit to the Institute, we announced, that, from the surface of the earth to the height of 3977 metres (13024.675 feet), the magnetic power did not experience any sensible diminution; and at the same time we signified a desire to undertake new ascents, to ascertain this important fact at greater heights. We soon found that this was also the wish of many members of the Institute; and, encouraged by the general interest which our first voyage had excited, we resolved on making a second very shortly; but our aerostat not being capable of carrying us together to a greater height than in our first ascent, it was agreed between us that I should go alone. From this moment all our attention was given to the instruments which it was requisite for me to carry; and their construction, which was again entrusted to M. Fortin, as well as an operation which the balloon underwent to give it more levity, retarded my departure till the 29th of last Fructidor.

Construction of the instruments.

Instructed by the experience of our former ascent, we made some changes in our instruments; and, in the first place, that the oscillations of the horizontal needle might be less affected by the rotation of the balloon, we caused a new one to be constructed, only 15 centimetres in length. Thus, its oscillations being more rapid than those of the balloon, it would be easier to determine their duration.

Horizontal needle.

Dipping needle.

We made greater modifications in the dipping needle. That we might not be obliged, at each observation, to replace it in the magnetic meridian, and to adjust its axis truly horizontal, we suspended the metallic chape which supports it by a silken thread; and, to judge of its inclination, we fixed a portion of a transparent circle, graduated in divisions, to the chape. The whole apparatus was very light, the tension on the silk thread but trifling, and the needle could resume its

* From *Annales de Chimie*, No. 154. Fructidor, An. XIII.

meridian

meridian with facility. M. Coulomb, after having touched the needle, proved it by the method which he proposed in the Memoirs of the Institute; and he found that it gave an inclination of 70.5° of the common scale. In one of its positions, which was that in which it was to remain, it varied 31° .

In our first ascent, the under surface of the glass which ^{Asimuth compass.} covered our azimuth-compass was covered with water, and we were prevented from seeing the shadow of a horizontal thread, which served us for a style. To avoid this inconvenience, it was sufficient to remove the glass of the compass; in other respects, no alteration was made in its first disposition. M. Lepine again supplied us with two second-watches, one of which was a stop-watch: it was with the latter that I made all my observations.

The thermometer I used, was the centigrade mercurial Thermometer. thermometer. To guard it from the action of the sun, we placed it within two concentric cylinders of pasteboard covered with gilt paper, one of which was about four centimetres in diameter, and the other six. Our hygrometers ^{Hygrometers.} with four hairs, of the construction of M. Richer, were disposed in nearly a similar manner. The two glass balloons in which I was to bring the air, were exhausted to nearly a millimetre of mercury, and we were satisfied by leaving them in this state for eight days, that they perfectly retained the vacuum. For fear of an accident, we had provided a third balloon of brass; but very fortunately it was useless.

Our two barometers have not a constant level; and to get ^{Barometers;} the true barometric heights, we formed a table of comparison, by placing them under the receiver of the pneumatic machine, and measuring their depression from five to five centimetres, by means of a standard, the reservoir of which has a constant level, and which is provided with a very good scale. Being no longer obliged to observe more than the upper level to get the true barometric heights, the number of observations was diminished to one half; which is of great importance when the attention is divided between personal safety and delicate experiments.

Such nearly were the essential instruments I took with me in my voyage. I was also provided with an apparatus for ^{The apparatus} determining the electricity of the air; but a few moments after for measuring the electricity of the air rendered ^{the electricity of} I quitted the earth, I lost the two metallic wires which were ^{the air rendered} ^{to useless.}

to attract the electricity at 50 and at 100 metres below me, and I could not make any use of it. It will be seen that we had avoided every thing which could influence our needles; and even our anchor, although suspended 50 metres below the boat, was of wood armed with copper.

This is not the place to mention the precautions which M. Conté had taken, that this new ascent might be free from danger; but it is to be wished that he would himself publish what a long and enlightened experience has taught him on this subject. For ourselves, we owe him a great tribute of gratitude for his trouble and the interest he has taken in our voyages; and we are bound to acknowledge, that if they have been so fortunate, we are indebted for it to his provident cares.

Departure.

All our instruments being ready, my departure was fixed for the 29th Fructidor: In fact, on that day I ascended from the Conservatory of the Arts and Manufactures, at 9^h 40', the barometer being at 76.525 centimetres, the hygrometer at 57.5°, and the thermometer at 27.75°. M. Bouvard, who makes meteorological observations daily at the Observatory of Paris, judged the atmosphere to be greatly charged with vapour, but without clouds. In fact, I had scarcely risen 1000 metres when I perceived a light vapour diffused through all the atmosphere, which rendered the sight of remote objects confused.

State of the atmosphere on the earth.

First experiment on the oscillations of the horizontal needle.

Arrived at the height of 3032 metres (9929.8 F.), I began to make the horizontal needle oscillate, and this time I obtained 20 oscillations in 83", while on the earth, and in other respects in the same circumstances, it required 84.33" to make the same number*. Although my balloon was affected with the rotatory motion we had perceived in our first experiment, the rapidity of the motion of our needle permitted me to count 20, 30, and even 40 oscillations.

Inclination of the needle at a height of 3032.365 feet, the same as on the earth.

At the height of 3863 metres (12651.365 F.), I found the variation of my needle, taking the mean of the amplitude of its oscillations, was sensibly 31°, as on the earth. It re-

* Although I have here indicated hundredth parts of a second, I am aware that I cannot observe such small fractions; but I subdivided them by divisions, because on the earth I generally made 30 oscillations, which required 126.9".

quired much time and patience to make this observation; because, although borne by the mass of the atmosphere, I perceived a slight wind which continually deranged the compass, and after several unsuccessful attempts, I was obliged to give up making new ones. Nevertheless, I believe the observation I have stated above is entitled to some confidence.

Some time after I wished to observe the dipping-needle; but behold what had happened. The drought, favoured by the action of the sun in a rarefied air, was such, that the compass was so much warped as to have bent the metallic circle on which the divisions were traced, and to be itself crooked.

The observations with the anemometer compass not to be depended upon.

The motions of the needle therefore had not the same freedom; but independently of this disappointment, I had observed that it was very difficult to observe the dip of the needle with this apparatus. In short, when I placed the compass so as to make the shadow of the horizontal thread, which served as a style, coincide with a fixed line, the motion I had given to the compass communicated one to the needle, and by the time this was nearly restored to rest, the shadow of the style no longer coincided with the fixed line. It was necessary to place the compass again in a horizontal position, and during the time which this operation required, every thing was again deranged. Unwilling to persist in making observations on which I could place no reliance, I abandoned them entirely; and divested of every other care, I gave all my attention to the oscillations of the horizontal needle. I am nevertheless convinced, by the knowledge of the defects of our compass, that it is possible to employ one more suitable, which would determine the dip with great precision. In trying this experiment, I lowered the other needles separately in linen bags, to 15 metres below the boat.

Cause of its defects.

The experiment still practicable.

That the aggregate of all the results which I obtained may be more easily seen, I have collected them into a table which is at the end of this memoir; and they are placed there as they occurred to me, with the corresponding indications of the barometer, thermometer, and hygrometer. The heights have been calculated, according to the formula of M. Laplace, by M. Gouilly, engineer of bridges and highways, who has been so kind as to take this trouble. The barometer not having varied sensibly on the day of my ascent, from ten o'clock to three, to calculate the different heights at which I

The aggregate of the results formed into a table.

Calculation of the heights by the barometer compared with one on the earth.

made

made my observations, the height of the barometer on the earth has been taken at 76.568 centimetres, which was the case at three o'clock; a height which, conformably to the observations made by M. Bouvard at the Observatory, is greater by 0.43 millimetres than that which had been observed at the moment of my departure. The heights of the barometer in the atmosphere have been brought to those which would have been indicated by a barometer with an uniform level placed in the same circumstances, and the mean between the observations of the two barometers has been taken for each height. The temperature on the earth having also varied very little between ten and three o'clock, it has been supposed constant and equal to 30.75° of the centigrade thermometer.

and of the temperature.

The irregular variation of the temperature arises from the change of place during the observation.

Difference of elevation corresponding to a degree of the difference of temperature.

Now on looking into the table it will be evident, in the first place, that the temperature follows an irregular course with respect to corresponding heights; which, no doubt, is occasioned by the observations having been made sometimes in ascending, and sometimes in descending, and the thermometer having followed these variations too slowly. But if the degrees of the thermometer alone are considered, which form with each other a continually decreasing series, a more regular law will be found. Thus the temperature on the earth being 27.75° , and 8.5° at the height of 3691 metres, if the difference of the heights is divided by that of the temperatures, we shall at once obtain 191.7 metres (627.8175 feet) of elevation for each degree of the reduction of the temperature. By performing the same operation for the temperatures 5.25° and 0.5° , as well as for those of 0° and -9.5° , we shall find, in both cases, 141.6 metres (463.74 feet) of elevation for each degree of the reduction of the temperature; which seems to indicate that near the surface of the earth the heat is governed by a law which decreases less than at an elevation in the atmosphere, and that at length it follows a decreasing arithmetical progression. If it be supposed that from the surface of the earth, where the thermometer was at 30.75° to the height of 6977 metres (22849.675 feet), where it had fallen to -9.5° , the heat had diminished as the elevations increased, an elevation of 173.3 metres (567.557 feet) will correspond to each degree of the reduction of the temperature.

The

The course of the hygrometer was very singular. At the ^{Unsteady course} surface of the earth it was only at 57.5° , while at the height ^{of the hygro-} of 3032 metres it marked 62° : from this point it was continually falling to the height of 5267 metres, where it indicated no more than 27.5° ; and from thence to the height of 6884 metres, it rose gradually to 34.5° . If, by these results, it is wished to determine the law of the quantity of water dissolved in the air at different elevations, it is clear that attention must be paid to the temperature; and on joining this consideration with it, it will be seen that it follows an extremely decreasing progression.

If now the magnetic oscillations are considered, it will be observed that the time for ten oscillations, made at different heights, is sometimes above and sometimes below $42.16''$, which they require upon the earth. By taking the mean of all the oscillations made in the atmosphere, ten oscillations would require $42.20''$, a quantity which differs very little from the preceding; but on considering only the last observations which were made at the greatest heights, the time for ten oscillations would be a little less than $42.16''$, which would indicate, on the contrary, that the magnetic power had been a little augmented. Without meaning to draw any consequence from this apparent slight increase, which may very probably arise from the errors to which experiments of this description are liable, I conclude that the ^{Magnetic oscil-} results I now offer, confirms and extends the fact which ^{lations at differ-} Biot and myself had observed, and which, as well as the ^{ent heights,} universal gravitation, proves that the magnetic power does ^{The magnetic} not experience sensible variations at the greatest heights to ^{power is not sensibly altered at the greatest elevations.} which we can ascend.

The consequence we have drawn from our experiments, ^{Probable objections to this conclusion refuted,} will appear a little too precipitate to those who recollect that we were unable to make experiments on the inclination of the touched needle. But if it be observed that the power which causes an horizontal needle to oscillate, is necessarily dependent upon the intensity and direction of the magnetic power itself, and that it is represented by the co-sine of the angle of inclination of this latter power, they cannot avoid concluding with us, that, since the horizontal power has not varied, neither can the magnetic power have varied, unless they suppose that the magnetic power can have varied precisely

cisely in opposition to, and in the same proportion as the cosine of its inclination, which is by no means probable. In support of our conclusion we have, besides, the experiment on the inclination made at the height of 3863 metres (12681.325 feet), which proves that, at that elevation, the variation did not vary in a sensible manner.

Further proof of the magnetism being the same as on the earth.

When at the height of 4511 metres, I presented to a small touched needle, and in the direction of the magnetic power, the lower extremity of a key; the needle was attracted, and afterwards repelled by the other extremity of the key, which I brought down parallel to itself. The same experiment, repeated at 6107 metres, had the same issue: another very evident proof of the action of terrestrial magnetism.

Heights at which the air was taken for examination.

At the height of 6561 metres (21487.275 feet), I opened one of my two balloons of glass; and at 6636 metres (21733.9 feet), I opened the second: the air entered both with a hissing noise. Finally, at 3^h 11', the aerostat being perfectly full, and having only 15 kilogrammes of ballast, I determined to descend. The thermometer was then at 9.5° below the temperature of melting ice, and the barometer at 32.88 centimetres; which gives for my greatest elevation above Paris, 6977.37 metres (22850.887 feet), or 7010 metres (22977.4 feet) above the level of the sea.

Greatest elevation.

Effects on the animal economy at this height.

Although well clothed I began to feel the cold, particularly in my hands, which I was obliged to keep exposed to the air. My respiration was sensibly obstructed; but I was far from feeling any inconvenience sufficient to induce me to descend. My pulse and my respiration were greatly accelerated; hence from respiring very frequently in a very dry air, I could not be surprised at my throat being so dry, that it was with difficulty I swallowed bread. Before setting out I had a slight head-ach, arising from the fatigues of the preceding day and the want of rest in the night, and I retained it all the day, without perceiving that it was increased. These are all the inconveniences I experienced.

Clouds at a very great height during this ascent.

Their extreme height in the furthest one.

A phenomenon which struck me at this great height, was the facing clouds above me, and at a distance which appeared to me to be very considerable. During our first ascent the clouds did not rise above 1169 metres (3828.475 feet), and above, the sky was in its greatest purity. Its colour at the zenith was so intense, that it might be compared with that of

Prussian

Prussian blue; but in the last voyage which I made I saw no clouds below me; the sky was very vaporous, and its colour generally dull. It is perhaps not useless to observe, that on the day of our first ascent the wind was north-north-west, and on the last, south-east. Direction of the wind.

As soon as I perceived that I began to descend, I thought only of moderating the descent of the balloon, and of rendering it extremely slow. At 3^h 45' my anchor touched the ground and held, which gives 34' for the time of my descent. The inhabitants of a small hamlet in the vicinity soon ran to the spot, and while some brought the balloon towards them by drawing the rope of the anchor, others, placed under the boat, waited impatiently till they could reach it with their hands, to catch and deposit it upon the earth. My descent was therefore effected without the slightest shock or the least accident, and I do not believe that a more fortunate one is possible. The small hamlet beside which I descended, is called Saint-Gourgon; it is situated six leagues north-west from Rouen. Descent.

On my arrival at Paris, my first care was to analyze the air I had brought. All the experiments were made at the Polytechnic School, under the eyes of MM. Thenard and Gresset, and I trusted as much to their judgment as to my own. We noticed the divisions of the eudiometer alternately, and without communication, and it was only when we were perfectly agreed that we wrote them down. The balloon which was filled with air at 6636.5 metres (21735.537 feet), was opened under water, and we all judged that it had at least filled the half of its capacity; which proves that the balloon had retained the vacuum very well, and that no foreign air had entered it. We purposed to have weighed the quantity of water entered into the balloon, to compare it with its capacity; but not having the necessary instruments at hand, and our impatience to know the nature of the air it contained being very great, we did not make that experiment. At first we used Volta's eudiometer, and we analyzed it comparatively with atmospheric air taken in the middle of the courtyard of the Polytechnic School. The following is the comparative analysis of these two airs. Analysis of the air taken at great elevations.

AEROSTATIC VOYAGE.

*Analysis of the atmospheric
Air.*

*Analysis of the Air taken at an
Elevation of 6636 Metres.*

	Measures.		Measures.
First Ex- periment.	Atm. Air, 3.	Air, -	3.
	Hyd. Gas, 2.	Hyd. Gas,	2.
	Residue after the combus- tion, -	Residue,	3.05
	3.04		
<hr/>			
	Measures.		Measures.
2d Expe- riment.	Atm. Air, 3.	Air, -	3.
	Hyd. Gas, 2.	Hyd. Gas,	2.
	Residue, 3.05	Residue,	3.04.

Atmospheric air
and air taken at
a great elevation,
are identically
the same.

At the same time one measure of very pure oxygen gas required 2.04 measures of nitrogen gas; and this result differing only .01 from that obtained in experiments made on a very large scale, and with great care, in the composition of water, it is evident that great confidence may be placed in our results. They prove therefore that atmospheric air, and air taken at an elevation of 6636.5 metres, are identically the same, and that each of them contains .2149 of oxygen. On analyzing the latter air by hydro-sulphuret of potash, we found in it .2163 of oxygen. I cannot give the result of the comparative experiment made on atmospheric air, because we were unable to collect it; but the proportion of oxygen which I have indicated, is also a little greater than that given by the combustion of hydrogen gas, and is comprized within the limits of the variations found in the composition of the atmosphere at the surface of the earth, and which do not prevent it from being considered as constant.

This fact proved
by other experi-
ments.

The identity of the analyses of the two airs made by hydrogen gas, proves directly, that that which I brought did not contain the latter gas; nevertheless I also satisfied myself of it, by burning with the two airs only a quantity of hydrogen gas less than would have been requisite to absorb all the oxygen gas; for I found that the residues of the combustion of the two airs with the hydrogen gas were exactly the same.

Saussure junior also found, by using nitrous gas, that air taken in the Col-du-Geant contained, within one hundredth part, as much oxygen as that of the plain; and his father ascertained the presence of carbonic acid on the summit of Mont-Blanc: Moreover, the experiments of MM. Cavendish, Macarty, Berthollet, and Davy, have confirmed the identity of the composition of the atmosphere over all the surface of the earth. It may therefore be concluded generally, that the constitution of the atmosphere is the same from the surface of the earth to the greatest heights to which we can ascend.

These are the two principal results which I obtained from my last voyage. I have ascertained the fact which had been observed by M. Biot and me, on the sensible permanence of the intensity of the magnetic power at a distance from the surface of the earth; and besides, I think I have proved that the proportions of oxygen and azote which constitute the atmosphere, do not vary sensibly in the most extended limits. Many things still remain to be elucidated in the atmosphere, and we hope the facts we have already collected will so far interest the Institute as to induce them to desire us to continue our experiments.

TABLE OF OBSERVATIONS.

Temperature expressed in degrees of the centigrade thermometer.	Mean of the indications of the two thermometers.	Mean height of the barometer in the atmosphere brought to that of a barometer with a constant level.	Corresponding heights in metres above Paris.	Corresponding heights in English Feet.	Number of magnetic oscillations found.	Duration of oscillations expressed in seconds.	Oscillations reduced to the common number 10.	Correcting times.	OBSERVATIONS.
27.75	57.5	Cent. 76.525	0.0	0.0	30	126.5	10	42.16	The first line gives the observations
12.50	62.0	53.81	3032.01	9929.803	20	83.0	10	41.5	which were made on the earth before the departure.
11.00	60.0	51.43	3412.11	11274.660					
8.50	57.5	49.68	3691.32	12089.073	10	42.0	10	42.0	
10.50	53.0	49.05	3816.79	12499.987	30	127.5	10	42.5	
		45.28	4511.61	14775.522	30	125.5	10	41.8	
12.0	50.9	46.66	4264.65	13966.728	30	86.0	10	43.0	
10.0	29.9	46.26	4327.86	14173.741	20	94.5	10	42.2	
8.25	27.6	44.04	4725.90	15469.322	20	128.5	10	42.8	All the heights must be augmented by at least 39 metres (20 toises), if it is wished to reckon them from the level of the sea.
6.50	27.5	43.53	4808.74	15746.200	30	127.5	10	42.5	
8.75	29.4	45.28	4511.61	14775.522	30	169.0	10	42.2	
5.25	30.1	42.49	5001.85	16381.038	40				
4.25	27.5	41.14	5267.73	17251.815					
2.5	32.7	39.85	5519.16	18069.249					
0.5	30.2	39.01	5674.85	18585.133					
1.0	33.0	41.41	5175.06	19498.321	30	126.5	10	42.1	
	32.4	37.17	6040.70	19783.292					
—	32.1	36.96	6107.19	20001.047	20	84.0	10	42.0	
—	33.1	39.78	5631.65	18387.343	30	127.5	10	42.5	
—	33.9	36.70	6143.31	20119.340	20	82.0	10	41.6	
—	34.5	33.39	6894.14	22545.558	20	83.5	10	41.7	
—	—	32.88	6977.37	22850.887					

Experiments

XIII.

Experiments and Observations on the Motion of the Sap in Trees.

*In a Letter from THOMAS ANDREW KNIGHT, Esq. to the
Right Hon. SIR JOSEPH BANKS, Bart. K. B. P. B. S.
From the Philosophical Transactions for 1804.*

MY DEAR SIR,

IN the Observations on the Descent of the Sap in Trees, which I last year took the liberty to request you to lay before the Royal Society, I offered a conjecture, that the vessels of the bark, which pass from the leaves to the extremities of the roots, were, in their organization, better calculated to carry the fluids they contain towards the roots than in the opposite direction. I had not, however, at that time, any experiment directly to support this supposition; but I thought the forms generally assumed by trees in their growth, evinced the compound and contending actions of gravitation, and of an intrinsic power in the vessels of the bark, to give motion to the fluid passing through them. In the account of the experiments which I have now the honour to address to you, I trust I shall be able to adduce some interesting facts, in support of that inference.

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Having selected, in the spring of 1802, four strong shoots of the vine, growing along the horizontal trellis of my vinery, I depressed a part of each shoot, whilst it was soft and succulent, about three inches deep, into the mould of a pot placed beneath it for that purpose; but without making any wound, or incision, in the young shoots thus employed as layers.

In this position they remained during the succeeding summer; and, in the autumn, had nearly filled the pots, which were ten inches in diameter, with their roots. As soon as the leaves had fallen, the layers were disengaged from the parent stocks; and about five inches of wood, containing one bud, were left, both at the proper and the inverted end of each layer. Every bud was also, by previous management, made to stand at an equal distance from the mould in the pots, and with an equal elevation, of about thirty-six degrees. About one inch of wood was likewise left at each end of every layer beyond the buds.

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In the succeeding spring, the buds vegetated strongly, both at the proper and at the inverted ends of the layers, as the experiments of Hales and du Hamel had given me reason to expect; and, in one instance, the bud at the inverted end of the layer grew with greater vigour than that at its proper end: but the growth of these buds was not the object which I had in view.

I have already stated, that nearly an inch of wood was left at each end of every layer, beyond the bud; and, to this wood, at the inverted ends of the layers, my attention was chiefly directed: for, if the vessels of the bark possessed the powers I attributed to them, I concluded that the sap would be impelled to the inverted ends of the layers, and be there employed in the production of new wood, and roots; and, in this, my expectations were not disappointed. At the proper end of the layers, the wood immediately beyond the buds became dry and lifeless, early in the succeeding summer; the stems also, between the buds and the mould in the pots, increased in size as usual; and nothing peculiar occurred. But, at the inverted end, appearances were extremely different: new wood here accumulated rapidly beyond the buds, and numerous roots, of considerable length, were emitted, whilst no sensible growth took place between the base of the young shoots and the mould in the pots.

It having been proved by Du Hamel, that inverted parts of trees readily emit roots, I expected to derive further information from cuttings of this kind: I therefore planted, in the autumn of 1802, forty cuttings of the gooseberry-tree, and an equal number of the common currant-tree; one half of each being inverted. Of the former, not one of the inverted cuttings succeeded; whereas few of the latter failed; and in these I had an opportunity of observing the same accumulation of wood above the bases of the annual shoots, and the same mode of growth, in every respect, as in the inverted vines; except that no roots were emitted at their upper ends. The same thing occurred, without any variation, in inverted grafts of the apple-tree.

If it be admitted, according to the theory I have on a former occasion laid before you, that the sap descends from the leaves through the vessels of the bark, and that such vessels are, in their organization, better calculated to carry their contents towards the original roots than in the opposite direction, it will be extremely easy to explain the cause of the accumulation of wood,

wood, and the emission of roots, above, instead of below, the base of the annual shoots. The vessels of the bark (the *vaisseaux propres* of Du Hamel) commencing in the leaves, were formerly traced by M. Mariotte, and subsequently by myself, (being ignorant of his discovery,) to the extremities of the roots; and, when a cutting or tree is planted in its natural position, the sap passes downwards through these, to afford matter for new roots, and to increase the bulk of those already formed, having given proper nutriment to the branches and trunk in its descent. But, in the inverted cutting, or tree, these vessels become inverted; and, if their organization be such as I have supposed it, a considerable part of that fluid, which naturally descends, will be carried upwards, and occasion the production of new wood, above, instead of below, the junction of the annual shoot with the older wood, as in the experiments I have described. The force of gravitation will, however, still be felt; and by its agency, sufficient matter to form new roots may be conveyed to those parts of the inverted cutting, or tree, which are beneath the soil. Besides, if we suppose a variation to exist in the powers or organization of the vessels which carry the sap towards the root, we may also attribute, in a great measure, to this cause, the different forms which different species or varieties of trees assume; for, if the fluid in these vessels be impelled with much force towards the roots, little matter will probably be deposited in the branches, which, in consequence, will be slender and feeble, as in the vine; and there is not any tree that has been the subject of my experiments, in which new wood accumulated so rapidly at the upper end of inverted plants. To an excess of this power, in the vessels of the bark, we may also ascribe the peculiar growth of what are called weeping trees; for, by this power, the effects of gravitation will be, in a great degree, suspended; and the pendant branch will continue healthy and vigorous, by retaining its due circulation. The perpendicular branch will, however, still possess some advantages; for in this, gravitation will act on the fluid descending from the leaves; and these will of course absorb from the atmosphere with increased activity. A greater quantity of matter will therefore enter, within any given portion of time, into vessels of the same capacity; and this increased quantity may frequently exceed that which the vessels of the bark are immediately prepared to carry away. Much new wood will in consequence be generated,

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and increased vigour given; and, the same causes operating through successive seasons, will give the ascendancy we generally observe in the perpendicular branch.

In the preceding experiments, none of the layers, or cuttings, exceeded a few inches in length; and, to the summit of these the sap appeared to rise, through the inverted tubes of the wood, nearly as well as in those which retained their natural position. But some former experiments had induced me to suspect, that this would not be the case in longer cuttings; I therefore planted in the autumn of 1802, twelve cuttings of the swallow, (*Salix caprea*,) inverting one half of them. The whole readily emitted roots, and grew with luxuriance; but their modes of growth were extremely different. In the cuttings which stood in their natural position, vegetation proceeded with most vigour at the points most elevated; but in the inverted cuttings, it grew more and more languid as it became distant from the ground, and nearly ceased, towards the conclusion of the summer, at the height of four feet. The new wood also, which was generated by these inverted cuttings, accumulated above the bases of the annual shoots, as in the preceding instances.

These facts appear to prove, that the vessels of plants are not equally well calculated to carry their contents in opposite directions; and, I think, afford some grounds to suspect that the vessels of the bark, like those which constitute the venous system of animals, (to which they are in many respects analogous,) may be provided with valves, whose extreme minuteness has concealed them from observation.

The experiments, and still more the plates, of Hales, have induced naturalists to draw conclusions in direct opposition to the preceding. But the Plates of that great naturalist are not always taken correctly from nature; * and Plates, under such circumstances, however fair and candid the intentions of an author may be, will too often be found somewhat better calculated to support his own hypothesis, than to elucidate the facts he intends to state.

The preceding peculiarities in the growth of inverted cuttings, appear to have escaped the observation of Du Hamel; and, as very few instances of error, or want of accurate observation,

* The eleventh Plate (Vegetable Statics) is that to which, in this place, I particularly allude.

will ever be found in the works of that excellent naturalist, I must request permission to send you some of the subjects of my experiments, as vouchers for my own accuracy.

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Of the inverted cuttings employed by Du Hamel, a small portion only appears to have remained above the ground; and under such circumstances, the different forms of those growing in their natural, or inverted, position would be scarcely observable. It appears also, from his experiments, that such inverted cuttings, in subsequent years, grow with as much vigour as others that are not inverted; whence we must conclude, that the organization of the internal bark becomes again inverted, and adapted to the position of the branch. The growth of some inverted plants of the gooseberry-tree, which I obtained, many years ago, from layers, gave me reason to draw a different conclusion; for these always continued weak and dwarfish. I do not, however, entertain the slightest degree of doubt, but that the assertion of Du Hamel is perfectly correct.

I intended to have added some observations on the reproduction of buds and roots of trees; but these would necessarily extend the present Paper to an immoderate length; I shall therefore reserve them for a future communication, and conclude with an account of an experiment which more properly belongs to the Paper I had the honour to address to you last year, but which had not then succeeded.

I have stated, in that Paper, that the leaf-stalk, the fruit-stalk, and the tendril, of the vine, had been successfully substituted, in many instances, for each other; but that I had failed in my efforts to engraft a bunch of grapes, by approach, on the leaf-stalk; owing, I conceived, to the operation having been improperly performed. In those experiments, I cut the leaf-stalk into the form of a wedge, and made an incision in the fruit-stalk, adapted to receive it; but, under such circumstances, the leaf-stalk (as I had proved by many experiments) has no power to generate new matter; and the wounds of the fruit-stalk heal so slowly, that I readily anticipated the ill-success of the operation. In the last spring, I pared off similar portions of the leaf-stalk and fruit-stalk; and, bringing the wounded parts into contact, I secured them closely together, by means of a bandage, letting the leaf remain. Under these circumstances, an union took place; and the fruit-stalk being then taken off below the point of junction, and the leaf-stalk above it, the grapes drew their

Experiments and observations on the motion of the sap in trees. their whole nutriment through the remaining part of the leaf-stalk. They did not, however, acquire their full size; and the seeds were small, and, I think, incapable of vegetating; but this I attribute to the want of nutriment in quantity rather than in quality; for the union of the vessels of the leaf-stalk with those of the fruit-stalk was very imperfect. The grapes, which were the purple Frontigniac, possessed their musky flavour, in the same degree with others growing on the same plant.

There is another experiment in my last Paper, which I will also notice here, because it appears to lead to some important conclusions, and had been tried only in a single instance. I have there stated, that the stem of a young tree became elliptical, by being confined to move only in the segment of a large circle. This experiment was successfully repeated during the last year, on other trees; but I have nothing to add to the description which I have already given.

I am, &c.

T. A. KNIGHT.

XIV.

Extract of a Memoir of Mr. ERMAN, entitled, Observations and Doubts concerning Atmospheric Electricity.*

MR. ERMAN who is already so advantageously known to philosophers, has published towards the end of last year, a memoir which appears to deserve their attention in an uncommon degree.

Signs of atmospheric electricity differ according to the instrument.

When he made experiments upon the electricity of the atmosphere, he observed a great difference between the result presented by an electrometer armed with a conductor which was suddenly raised from the earth, according to the known method, and that of a metallic rod of much greater length which was insulated and fixed.

Description of the electrometer.

The electrometer he used was that distinguished in Germany by the name of the electrometer of Weiss. The length of its leaves of gold is half an inch, and the diameter of the glass tube which encloses them is three quarters of an

* Journal de Physique, Thermidor 12, or Vol. LIX. 98.

inch in diameter, the height being an inch and a half. Its cover of ivory does not project above the glass, and is perforated in the middle with a hole in which a smaller glass tube is fixed, and through this last tube passes the metallic rod that serves to suspend the gold leaves, and is upon the whole five feet in length, being composed with several pieces screwed together, in order that they may be more easily conveyed from place to place by separating them. A divergence of three lines in these leaves answers to two lines in the electrometer of Volta; and that degree of electricity which would produce a divergence of two lines and a half in this last, causes the leaves of Mr. Weifs to touch the sides of the glass cylinder.

Mr. Erman walked in the fields with his electrometer, having its rod confined to the length of three feet. When he suddenly raised it from the ground, he observed a strong divergence of *plus* electricity. When he lowered it with the same swiftness, he perceived an equal great divergence of *minus*. The point of a fixed metallic rod of much greater length, erected at the same spot, did not give the least indication of electricity. When raised suddenly it diverges *plus*.

An electrometer placed on a post of three feet in length, produced no divergence; and, when it was afterwards slowly raised one foot, or a foot and a half, it likewise afforded no sign; but when it was very quickly brought down to the post, it exhibited *minus* electricity to such a degree, that the gold leaf touched the sides of the glass. This charge was gradually dispersed in the air, or by touching the electrometer with the hand*; but by raising the instrument with equal speed above the post, a divergence of *plus* electricity of the same strength was afforded which was destroyed in the same manner. If slowly raised it does not diverge; but if then suddenly depressed it diverges *minus*.

The more insulating the air the less it is necessary to raise the electrometer in order to produce this effect; but the longer the point, the more strikingly will the effect be shewn, even in damp weather. A circular motion, at a constant distance above the earth does not exhibit any electricity, neither does a progressive motion upon level ground afford any change; Conducting power of the air interrupts the results.

* From this fact it appears, that the electrometer had not any slips of metal within the tube like those of Bennet, who first used gold leaves. N.

but

but when the ground rises even insensibly, the phenomenon is observed, and it is rather extraordinary, that it even affords a means of ascertaining the level.

Variations of the experiment.

To vary the experiment, the electrometer may be placed upon a low support, and touched with an insulated stick or wire which has been quickly moved towards the ground, and is afterwards raised with the same quickness. The electrometer in the first case exhibits *plus* electricity, and in the second *minus*.

It is very remarkable, that the *minus* electricity passes through Zero before it changes into *plus*, and *vice versa*, accordingly as the instrument is raised or lowered even when there is no interval between the two motions; but the divergence will be more visible, if time be allowed for the electrometer to discharge itself between the two experiments.

Saussure's observations.

Saussure formerly observed the essential requisites in this phenomenon; but his electrometer was not so sensible, and he explained the phenomenon by supposing that the electrometer was the most insulated, and the fixed rod not sufficiently so to indicate such slight degrees of electricity.

The electrometer is affected not by any electricity in the atmosphere, but simply by the vicinity of the earth which appears to possess the *minus* state.

Mr. Erman has ascertained, that the phenomenon must be attributed to the manner in which the electricity is distributed in bodies. The mass of the ground exerts its influence very sensibly to a certain distance, and the electric atmospheres act upon each other without any intervention of the air. The insulated metallic rod which we raise from the ground by holding it perpendicularly, undergoes no other change than is owing to its own proper electricity, which is then less compensated by the influence of the ground. Whether the electrometer or the plate of an electrophorus be slowly raised, the electricity becomes communicated to the surrounding air,

The termination of the conductor or even its being enclosed in glass, makes no difference.

and is not manifested. A ball placed on the point of the rod of the electrometer does not at all change the divergence of the leaves; and even when the conducting rod of the electrometer is enclosed in a tube of glass not open at the top, the phenomena are the same, and the divergencies of *plus* and *minus* are equally seen according to the direction of the motion. And when a second tube is put over the first, for the purpose of more exactly preventing the contact of the air, the results are not at all changed.

Mr. Erman procured a tube of glass 14 feet long, which he covered with varnish, and through which he introduced a metallic wire which passed above the upper part, and had the electrometer adapted to its lower extremity. A small piece of amber being rubbed and applied to the upper end of the wire, produced a divergence in the leaves. The electricity of the air had not, however, any influence on this fixed, and very nicely insulated point; which shews, that the air surrounding a point is more adapted to deprive it of electricity than to communicate it. Another electrometer which was raised from the earth at the same time produced a great divergence.

A fixed rod not affected, because its situation with regard to the earth is not changed.

Mr. Erman wished to know whether two bodies in the same atmospheric stratum reciprocally modify their electric state, when their distance is altered in either direction. Two gold leaf electrometers equally sensible, and armed with rods three feet long, one of which was bended, so that its point could be placed near that of the other electrometer, on the same horizontal line were held at arms length from each other, and touched with the finger to discharge, and put them in equilibrium with the surrounding air. They were afterwards brought towards each other horizontally, and when they were sufficiently near to permit their atmospheres to act on each other, there was a divergence of *minus* electricity on both sides, which came to its maximum when the electrometers were in contact. They were again separated, and the divergence disappeared, both being at the state of Zero. One of the wires of these electrometers was put in contact with the earth, when the divergence of the other insulated electrometer suddenly diminished to half its former quantity; a fact that also proves that the whole depends upon the electricity inherent in bodies, and not at all upon that of the atmosphere.

Two electrometers brought into contact, and then horizontally separated affect each other according to the laws of bodies actually electrified.

When the electrometers were removed from each other, that which was touched with the finger indicated *plus* in a degree equal to the *minus* it had before shewn. If the two electrometers be touched with the finger, no further appearance of the *minus* divergence is seen in either of them; but if they be then separated both will exhibit *plus*. Can these phenomena be attributed to the air? or is it possible to avoid admitting in these experiments the influence of electric atmospheres, and the law of the condenser?—It is, therefore, well

Continuation of the experiment

well proved, that all bodies, even which are in equilibrium with the earth, have electric atmospheres in the open air, from which results a modification in their electric state.

The electric influence of a tree was still greater.

Mr. Erman conjectured, that a body of a greater volume might have still more influence. He observed a tree which stood alone in an open space. He retired to the distance of twenty paces from it, and discharged the electricity of his electrometer by touching it with the finger. He then approached the tree, keeping his electrometer at the same height above the earth. The electrometer diverged negatively, and the divergence continually augmented as he came nearer, so that the leaves of gold touched the sides of the glass at the moment when he was under the tree. This negative state lasted as long as he remained there, but when he retired again it gradually disappeared, so as to have become insensible when he arrived at the original distance of twenty feet.

When the communication between the point of the electrometer and the earth was completed under the tree, the divergence ceased; but the electricity was only apparently destroyed, and manifested itself again when he retired from the tree.

The mutual influence of two electrometers is scarcely seen very near the earth. Remarks on aerostatic experiments proposed.

Two electrometers being brought near each other at the distance of half a foot from the earth, did not produce this effect, because the mass of the earth too greatly influenced that proximity. In order to have very simple results, it would be requisite to discover some means of rapidly elevating an insulated body to some thousands of feet above the ground; and if this experiment were undertaken in the boat of a balloon, it would probably be seen, that the positive charge would constantly increase during the ascent, and would become negative as it approached the earth: but if means could be had of changing the system of the balloon in the atmosphere, by the intervention of another body, which would be difficult, the contrary effects might be expected. This change of system is however seen when a cloud descends, and discharges itself with explosion into the earth. But an electrometer of the usual construction in the boat of a balloon would indicate nothing, because the upper part always has a charge of electricity of the same kind as that of the gold-leaves, and of equal intensity.

If

If the influence of the electricity of the ground be sufficient to prevent the divergence, the same effect ought to take place in every close chamber, because a roof or ceiling may be considered as a prolongation of the ground. Hence it is that this polarity does not manifest itself in a chamber; it is also erroneous to pretend that the atmosphere produces electricity; and it is equally erroneous to assert, that electric repulsion does not take place in a vacuum; Mr. Erman proposes to establish in an incontestible manner in another memoir, that the repulsive force of electricity, as well as of magnetism, are also manifested in a vacuum.

If the electricity of the rod of the electrometer could be ascribed to that of the atmosphere, it would follow, that it should be equally electric through its whole length; but if it depend on the earth, the rod will not be equally electrified, and its different parts will be variously modified accordingly as they may be more or less remote from the earth; and this in fact is the case. Two electrometers, each having a pointed rod of three feet, one of which was twice bended in a right angle, so as to have an horizontal portion of three or four inches before it proceeded upwards, were held in such a manner, that the point of the straight rod of the one touched the horizontal part or elbow of the other, which was lower. When they were touched, to discharge them, no divergence was seen, but when they were afterwards separated horizontally, the leaves of the lower electrometer diverged, while those of the upper were not at all moved.

It is very remarkable that the nearer the contact is made to the upper point of the compound conductor to take its electricity from it, the greater is the positive divergence of the lower electrometer; and if the discharge be made very near the upper point, the electrometer will indicate *plus* but very feebly in comparison with that below.

If these two electrometers, the conducting parts of which form together a length of six feet, be raised perpendicularly some feet above the earth, the *plus* electricity will be observed in both; but if they be separated horizontally, the upper one will retain its divergence, and the lower will augment it suddenly, even to double. From this it is evident that the conductor has not the same charge of electricity through all its length, and this is precisely what is called polarity; and as the presence

These effects cannot take place in a chamber, &c.

The electric signs do not come from the air; for the rod is not alike electrified throughout.

Its state differs as the contact is made in different parts of its length.

Electric polarity.

presence of the electrometer is not essential in this experiment, we may conclude from it that each conductor placed perpendicularly on the earth, has its polarity in the direction in which we have just explained it.

One extremity of an electrometer indicates divergence; the other does not.

This shows why an electrometer indicates a divergence *plus* when the point is touched with the hand in the open air, and is soon withdrawn, while there is no divergence when the lower part of the wire is touched. This phenomenon is seen in Weiss's electrometer, even when the wire is not more than three quarters of a foot long.

Experiments on the electricity of the atmosphere uncertain.

From all this it will be seen how deceitful the experiments on the electricity of the atmosphere may be; for in the common method, the hand is directed to be moved from the bottom upwards, towards the point of the electrometer, to deprive it of its accidental electricity, and it is precisely this which communicates it to it.

The intervention of a cloud, rain, &c. reverses the phenomena;

With respect to the exceptions which the facts announced above may be liable to, it must be observed that when a storm drives a cloud over the zenith of the observer, or when rain, hail or snow falls at the place of observation, the phenomena which occur are totally opposite; the electrometer gives *plus* when it is brought towards the earth, and *minus* when it is removed from it, &c. &c. But this anomaly is only a transient effect, and the pretended negative state of the atmosphere does not exist, either when the rain is of long duration, or when the sky is entirely covered with clouds; for in the latter case the electrometer is positive on being raised, as when the sky is clear. The perturbations spoken of are only momentary and change continually. It is evident that these changes cannot be attributed to the charge of electricity set at liberty in the air: it is more probable that these masses of clouds modify the electrometer by their atmospheres, like the tree in the preceding experiment. The perpendicular conductor may be compared to an iron bar, the polarity of which is reversed accordingly as the same pole of a magnet is presented to one or other of its extremities; these effects of the meteoric masses may also be imitated by conductors; the sole motion of the hand above the point of an electrometer is sufficient to produce the negative state in question, and insulation renders these effects more sensible.

but a continued rain or a sky wholly obscured by clouds does not.

These changes occasioned by the atmosphere of the cloud,

and may be imitated.

(To be concluded in our next.)

SCIENTIFIC

SCIENTIFIC NEWS.

PROFESSOR DANZEL of Hamburg made, in the beginning of last year, a second experiment of the mechanism he has invented for the direction of aerostatic machines. He is said to have obtained a progress, in a right line, of twelve feet in a second, which is nearly equal to three leagues in an hour, although he did not bring more than half his means into action. On the same day he made an essay with another machine for the same use, the result of which was not less successful. He has since published the principles of his discovery in a pamphlet, entitled, "Basis of the Mechanism for the Direction of Aerostatic Globes, by Professor Danzel, of the Society of Emulation of Abbeville, at Hamburg."

Mechanism for directing aerostatic machines.

The class of physical and mathematical sciences of the National Institute of France, in its sitting of 2d Pluviose, in the year XII. elected the following corresponding members. Messrs. Leblond, engineer, returned from Cayenne; Bernard, engineer and astronomer, at Bagnols; Simons, physician at London; Crell, German chemist; Thunberg, the successor of Linnæus in Sweden, who made the voyage to Japan; Bugge, astronomer to the king of Denmark; Gosse, chemist at Geneva; Proust, chemist at Madrid; Cugnoli, astronomer at Modena; Reboul, to whom we are indebted for the measures made in the Pyrenees; Mendoza, a Spaniard, settled at London, known by his important works on navigation. They were all correspondents of the Academy of Sciences in 1793.

Correspondents elected by the class of physical and mathematical sciences of the French national Institute.

The same class has prolonged the time for receiving answers to the following prize questions, until the 1st Germinal, in the year XIII. "To determine by experiment the different sources of carbon in vegetables." And, "To determine by anatomical and chemical observations and experiments, what are the phenomena of the torpidity which certain animals, such as marmots, dormice, &c. experience during the winter, with respect to the circulation of the blood, respiration and irritability; to enquire what are the causes of this sleep, and why it is peculiar to these animals."

Prize questions.

The value of the prizes is doubled, and consists of two kilogrammes of gold, about 283l. sterling each.

The astronomical prize, instituted by M. de Lalande, to be given to the person, in France or elsewhere, the members of the

the

Prize, question. the Institute alone excepted, who shall have made the observation most interesting, or published the memoir most useful to the progress of astronomy, was in the public sitting of the 6th Messidor, decreed to M. Joseph Piazzi, Professor Royal of Astronomy and Director of the Observatory of Palermo, for the work which he has lately published under the title: *Præcipuarum stellarum inerrantium positiones mediæ ineunte sæculo XIX. ex observationibus habitis in specula Panormitana. Panormi, 1803, one volume in folio.*

The subject of the mathematical prize proposed by the class, in this sitting, is the following: To give the theory of the perturbations of the planet Pallas, discovered by M. Olbers." The works are to be written in French or in Latin; and are to be received to the 1st Germinal in the year XIV. The prize is a gold medal weighing a kilogramme.

The academy of sciences, literature, and the fine arts of Turin has proposed the following new prizes.

Class of Physical and Mathematical Sciences.

First Prize. The electric and galvanic fluids offer so many points of analogy, and so great a number of different effects, that many philosophers believe them to be identical, and many others make them two distinct fluids:

New experiments are required which shall decide, in a definitive manner, on their identity or diversity.

Second Prize. It will be seen in the *Connoissance des tems*, for the year XII. page 217, that the refractions which are adopted do not show an agreement in the observations of the summer and winter solstices of the years 7, 8, and 9, to give the same obliquity of the ecliptic, as they should do; and it is clear that a difference, such as is found, of eight seconds in the result of the calculations, not of one observation, or of two, but in the total of several, made on different days of different years, must have some cause:

A satisfactory explanation is demanded.

Class of Literature and the Fine Arts.

Question proposed. *To demonstrate whether the economical science known by the name of Statistics, is a new science, and what are the advantages states may derive from it.*

The prize for each question is 600 francs; and memoirs are to be received until the 30th Frimaire of the year XIII. They are to be written in Latin, French, or Italian, and

sent, free of postage, to the academy. The prizes will be ^{Prize questions} declared in the last public sitting (Messidor) of the same year.

The society of emulation of Colmar in its sitting of the 17th Thermidor, proposed the following subject for a prize of 300 francs, to be decreed in the public sitting of Messidor, in the year XIII.

" 1. What are the readiest and least expensive means of converting into animal oil, such remains of animals as have been hitherto lost to the arts and to consumption?"

" 2. To what arts, and to what kinds of fabrication can this product be applied; and what preparations should this substance undergo before it is brought into commerce?"

" 3. Finally, in an extensive undertaking, the object of which would be to convert the remains of animals into animal oil, what precautions would be necessary in the administrative police, to prevent the public health from being injured?"

The memoirs are to be written in French, German, or Latin, and sent before the 13th Messidor, free of postage, to the Prefect of the Department, or to the Secretary of the Society.

Extract of a Letter from Mr. J. DALTON, Lecturer at the Royal Institution. Dated, Manchester, March 12, 1805.

A VERY remarkable and singular appearance of the ^{Remarkable} *aurora borealis* was observed at Manchester from 10^{to} 11 in the evening of Saturday the 23rd of February. The sky was almost completely obscured with dark clouds, especially to the south, with some rain; about 60° above the horizon, on the south meridian, there was a space in which the cloud was less dense, where a very striking vacillating flame arrested the attention of most people who happened to be out. It sometimes shone with such vividness as to exhibit a stream of light right down to the horizon through the thickest part of the cloud; and at other times, the whole southern region was illuminated as with a flash of lightning. The light seemed scarcely to reach the zenith, and at breaks in the clouds to the north, no light appeared. Should this phenomenon have been observed in the south of England or in France, a comparison of the observations may lead us to form some idea of the height of this and other appearances of this meteor, which occur much more rarely of late years than formerly.

TO CORRESPONDENTS.

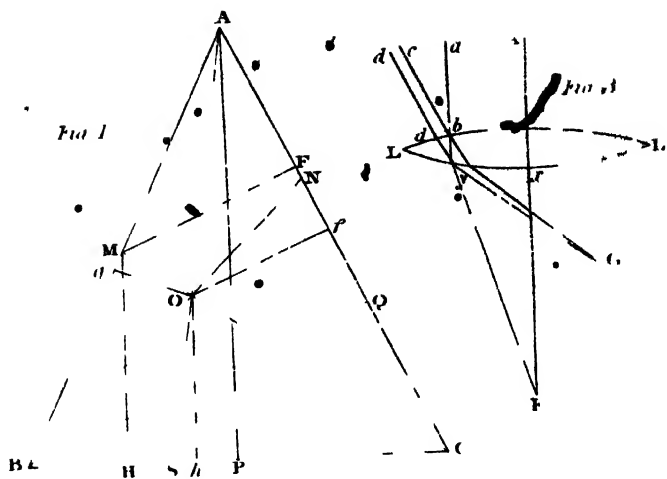
THE ingenious instrument of I. S. F. for explosions has not appeared to the Editor to be wanted. Chemical lecturers in this metropolis use a small particle of phosphorus, of the size of a pin's head, and sprinkle upon it a pinch of the oxymuriate of potash, in a Wedgewood's mortar. This small quantity, being struck by a sudden rub of the pestle, gives a loud explosion with the visible flame, but the burning matter is too little to endanger the spectators.

The excellent perspective instrument described and drawn by the Rev. WM. GREGOR, has no doubt the priority in actual date to that of R. L. EDGORTH, Esq. described in our first Vol. page 281. But he will see, by turning to that article, that the instruments are too nearly alike to admit of a second publication.

I am much obliged to Mr. CUMBERLAND for his favour of the 17th Feb. The useful objects mentioned in his postscript appear to me to be such as would be very acceptable to the public. I am afraid that the circumstance of the invention of Mr. J. having been published before would make it less proper to be reprinted in a Journal which is honoured with so much original matter, that presses for insertion. But I shall be happy to give it a more deliberate consideration if his friend will favour me with the loan of the plates, and such additional observations or notes as he may think fit to make.

The case of deafness, communicated by Mr. HILL of Wells, Norfolk, in which so much benefit was derived from electricity, is very interesting. Some medical cases have appeared in the early numbers of the first series of this Journal; and others presented themselves for my decision as Editor with regard to their value and importance. But in the attempt to perform this part of my duty, I found difficulties so great, and a subject so extensive, that it appeared proper to confine the papers, to be admitted, to such as are directly and immediately connected with chemical and mechanical science. I shall be happy to convey his letter to a respectable Medical Journal, if he should think fit.

I am very sorry that the letter from a CORRESPONDENT, who requested some enquiries to be made, concerning the fundamental experiments on heat, of the late DR. IRVING, has been so long without its effect. I hope to be enabled to give the narrative he requested in the next number.



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